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ABSOLUTE SCALING APPLIED TO THE DATA YIELDED BY THE METHOD OF CONSTANT STIMULI

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The results here to be presented constitute proof that when two magnitudes are presented in succession to an observer for comparison, these two stimuli do not act as independent variables, that is, as two magnitudes separately presented. If presented separately, each magnitude may be conceived as producing a variable effect upon the subject, *i.e.*, one which varies at successive presentations. The variation in this effect exerted by each of the two stimuli, 1 and 2, when presented singly, may be designated by the symbols σ_1 and σ_2. Now when the *two* stimuli are presented successively for comparison, it would seem that the outcome of the comparison should depend, primarily, upon the difference between the two compared magnitudes. The further supposition might therefore be made that the variability in the reported difference would represent the variability in a difference, the formula for which would be the well known one for uncorrelated variables, $\sigma_{diff.} = (\sigma_1^2 + \sigma_2^2)^{\frac{1}{2}}$ in which σ_1 and σ_2 represent the variability in the effect of each magnitude when presented separately. That this is by no means the case is the conclusion which the data to follow are believed clearly to demonstrate.

The data from which the calculations of variability were made consist of 64,500 comparisons instituted by the method of constant stimuli between short tonal durations, that is, short continuous tones. With a standard duration of 500 σ, were compared five variables differing from the standard by $-$ 20, $-$ 10, 0, $+$ 10, and $+$ 20 percent. The standard was presented equally often as the first and as the second of a comparison-pair. Forty-three subjects completed the series, which was divided into ten sittings of 150 comparisons each. These subjects had served about three months earlier in another comparison experiment as extensive as the one here reported, and in fact identical with it in all respects except as regards the length of the pause between the tones of a comparison-pair. In the first experiment, the pause was only 1.5 secs., while in the present instance it was 3.0 secs. In neither experiment was there any convincing evidence of improvement in acuity of discrimination with practice; but the first experiment showed, with prolongation of the sittings, certain pronounced shifts in the percentages of the various categories of reports.[1] Changes due to practice, whether those representing an increase in acuity or those other, sometimes more striking, changes which have been attributed[2] to the varying effect of 'remote standards,' would complicate the study of the relative variability of the various differences between the standard and the variables. It was surmised that the long practice already given the subjects (primarily for the purpose of studying changes in time-order errors) would result in data comparatively free from practice changes of any sort. While the data obtained were not entirely free from practice effects, they are more stable than those obtained from the first experiment and in this respect are, on the whole, well suited to the present purpose.

No description of the apparatus used to produce and to measure the durations is here necessary, since it has been

[1] For an account thereof, see "The effect of practice upon positive time-order errors" by H. Woodrow and L. H. Stott, *J. Exper. Psychol.*, 1936, **19**, 694–706.

[2] H. Woodrow, The effect of practice upon time-order errors, *Psychol. Rev.*, 1934. See also, Woodrow and Stott, *op. cit.*

described by Stott in his report [3] of an investigation concerning the indifference-interval for continuous tonal durations.

Since there were five variables, and each variable was used with the standard in both of the time-orders, V_1 and S_2 and S_2 and V_1 (S_1 meaning standard first and S_2 standard second), each sitting comprised ten comparison-situations. In the case of the zero-difference variable, however, the order V_1 and S_2 constitutes a situation identical with that of S_2 and V_1, both entailing comparison of the standard with itself. These two orders have therefore, in the case of the zero-difference variable, been amalgamated into one, which will hereafter be designated as S_1 and S_2. The others will be designated as $+$ 10 V_1 and S_2; S_1 and $+$ 10 V_2; etc. The allowed judgments, or reports, fell into three categories: equal, 2d longer, and 2d shorter. This number was reduced to two, by dividing the equal judgments, evenly, between the 2d longer and 2d shorter reports. For convenience, the percentage of 2d longer judgments, after inclusion of one-half the equal judgments, will hereafter be termed the percent score. There were thus obtained from each subject at each sitting nine percent scores. And since there were ten sittings, the entire experiment yielded in the case of each comparison-situation and each subject ten percent scores; and since there were 43 subjects who completed the experiment, the total 'population' of percent scores for each of the nine comparison-situations was 10 $\times$ 43, or 430.

Each of the percent scores represents a difference between a variable and the standard. The difference here referred to is of course the psychological difference, which may be termed, according to one's preference, an experienced or perceived difference, a discriminal difference, an effective difference, etc. The last term will hereafter be used in this paper. The magnitude of the difference, no matter by what term the difference is designated, is estimated solely by reference to the percentages of reports of various categories. The only evidence obtained in this study of any psychological difference

[3] L. H. Stott, Time-order errors in the discrimination of short tonal durations. *J. Exper. Psychol.*, 1935, **18**, 741–766.

lies in these percentages. The term effective difference is chosen because these percentages are so divided between the two categories 2d longer and 2d shorter as to indicate that they were produced or *effected* by a difference of a certain magnitude. In other words, the percentages are those which would be expected if the subject was correctly describing a difference of a certain magnitude and variability. The effective difference, then, is the difference which must be postulated on the assumption that all of the subject's reports are correct, or more simply, it is the difference indicated by the reports. This difference is never the difference between the physical magnitudes, a fact which is obvious, for one reason, because of the variable nature of the reports in spite of physical constancy in the comparison-situation. A given physical situation, for example + 10 V_1 and S_2, which constitutes the stimulus for a particular judgment of difference, produces a variable effect upon the subject. The physical difference between the two durations entering into the situation is constant, but the effective difference varies. When two sets of fifteen trials give identical percentages, it is assumed that the mean effective difference for those sets of 15 trials is identical. An increase in the percentage means an increase in the effective difference, and a decrease in the percentage, a decrease in the difference.

To determine the variability (standard deviation) in the absolute magnitude of the effective differences represented by the 430 percent scores obtained with each comparison-situation, was the chief purpose of the investigation. This has been accomplished by the use of two absolute scaling methods. Calculations of two other variabilities than the above mentioned one have also been made. These further calculations appeared desirable on account of the abstractness of the concept of a population of 430 differences consisting of 10 differences from each of 43 individuals. One of these additional variabilities is the variability in the 43 differences, when each difference is that represented by the mean percent score of one subject for the entire ten sittings. The other is the standard deviation for the average individual subject in his

10 mean differences, each of these differences being that represented by the percent score of a single subject in the case of one sitting and one comparison-situation.

Besides the variability of the effective difference produced by any of the nine combinations of physical stimuli, the absolute magnitude of these differences has also been determined, and located on a scale which, differing from most absolute scales, has an absolute zero. Thus, it is possible on the basis of the data to be presented, to show in one diagram both the mean magnitude, measured from an absolute zero, and the variability of the nine effective differences, with both the mean magnitudes and the variabilities measured in the same constant unit (see Fig. 4). It will be recognized at once that such a thorough picture of the outcomes of an experiment on comparison by the method of constant stimuli has never before been presented.

The distribution of the 430 percent scores for each comparison-pair is indicated, cumulatively, in Table I. The table is constructed in a manner to show in what proportion of each population of 430 percent scores the variable listed at the top of a column was indicated to be *longer* than the standard, in excess of any specified percentage. The various percentages are specified in the left-most column. In arriving at each proportion, one-half of the scores exactly equaling any given percent score were regarded as exceeding it and one-half as falling below it. In some comparison-situations, a number of the percent scores were zero. In the case of $-$ 20 V_2 and S_1, this number was so large as to render dubious the advisability of submitting the scores to any scaling technique, since with a large number of zero scores, it is impossible to determine the shape of a considerable portion of the distribution. However, the only indispensable condition is that not more than one-half the scores consist of zero, or, in the opposite case, of 100 percent. These results have therefore been treated the same way as those obtained with the other comparison-situations. In this case of $-$ 20 V_2 and S_1, 194 of the 430 percentages were zero; that is, 194 of the scores showed the standard to be always judged longer. Twenty-seven scores

were only 0.5, a fact which means that in 54 cases, except for one equal judgment, the standard was uniformly reported to be the longer duration. One-half of these scores of 3.3 percent (0.5) are regarded as below 3.3 percent and one-half as above it. Thus the total number of scores falling below 0.5 is regarded as 194 + (.5 × 27), or 207.5. Since 207.5 as a proportion of 430 equals .483, this proportion of the scores is regarded as falling below 3.3 percent, and the proportion exceeding 3.3 percent as 1 − .483, or .517, the latter proportion being the one given in the table. In the case of comparisons of the standard with itself, the proportions are listed twice. In the column headed $S_1 > S_2$ are listed the proportions when the scores consist of the percentage of trials showing the first duration to be judged the longer; and in the column $S_2 > S_1$, the proportions when the scores are taken as the percentage of judgments indicating the second to be the longer.

As a matter of fact, none of the columns in Table I should be headed by a single duration. The subjects' reports are not judgments of a single stimulus-duration, but of one duration

TABLE I

DISTRIBUTIONS OF THE POPULATIONS OF PERCENT SCORES

n, each population, = 430. Each percent score is based on 15 reports, except in the case of S_1 and S_2, in which case it is based on 30 reports. Total number of reports represented by the table is 64,500.

Percent Scores	Proportions of the populations of percent scores exceeding various percent scores									
	$-20\ V_1$	$-20\ V_2$	$-10\ V_1$	$-10\ V_2$	$S_2 > S_1$	$S_1 > S_2$	$+10\ V_1$	$+10\ V_2$	$+20\ V_1$	$+20\ V_2$
96.7	.000	.000	.000	.000	.000	.000	.022	.005	.059	.143
90.0	.000	.002	.000	.000	.000	.002	.065	.056	.151	.390
83.3	.000	.002	.006	.000	.001	.014	.156	.188	.299	.615
76.7	.006	.006	.013	.002	.009	.029	.262	.335	.424	.772
70.0	.015	.007	.035	.002	.030	.103	.438	.522	.597	.880
63.3	.034	.007	.079	.002	.079	.259	.579	.688	.743	.934
56.7	.055	.007	.149	.014	.195	.415	.730	.820	.831	.966
50.0	.102	.007	.244	.027	.377	.623	.835	.913	.908	.979
43.3	.198	.022	.419	.071	.585	.805	.920	.965	.957	.991
36.7	.306	.030	.585	.131	.741	.921	.973	.988	.980	.995
30.0	.458	.052	.752	.252	.897	.970	.992	.992	.991	.995
23.3	.629	.074	.860	.410	.971	.991	1.000	.998	.995	.998
16.7	.769	.149	.949	.621	.986	.999	1.000	.999	.995	.998
10.0	.897	.280	.984	.779	.998	1.000	1.000	1.000	.998	.999
3.3	.965	.517	.998	.926	1.000	1.000	1.000	1.000	1.000	1.000

in relation to another. The percent scores, therefore, are indications of the effective magnitude of *the difference between two durations*, not of the magnitude of either duration. A given report, *e.g.*, the report, longer, in response to the comparison-situation + 10 V_1 and S_2, may be equally well described as one indicating that the variable was indicated as shorter than the standard or that the standard was indicated as longer than the variable.

Any difference has a direction or sign, though the sign is arbitrary (except that if the sign of one difference is changed that of all must be changed). Thus if A is greater than B by an amount designated as $+ x$, then B is greater than A by $- x$. To reverse the description from $A > B$ to $B > A$ merely reverses the direction, or *sign*, of the difference, but does not affect its magnitude. It will be observed in Table I that when the score is for $S_1 > S_2$, the proportion of the scores exceeding any percent is always greater (except when zero or 100) than when the score is for $S_2 > S_1$, that is, the first of the two equal durations is judged longer more often than shorter. Zero difference between the compared physical magnitudes produces a small but significant effective difference. The magnitude of this difference is obviously the same whether it be described as $S_1 > S_2$ or $S_2 > S_1$, but if the sign of the first difference is taken as positive, that of the second becomes negative. It will arbitrarily be regarded as positive when described as $S_1 > S_2$. Further, all differences in which the variable is on the average reported greater than the standard will be arbitrarily regarded as positive; and those differences in which the variable is less than the standard, negative. Thus the difference $-$ 20 $V_2 > S_1$ is regarded as negative. If recorded as $S_1 > -$ 20 V_2, however, it would be positive. Accordingly, in the case of all differences considered positive, Table I shows over 50 percent of the scores exceeding the score 50 percent; while in the case of all those considered negative, it shows less than 50 percent of the scores surpassing the score 50 percent.

The data of Table I may also be represented by distribution surfaces, as in Fig. 1. The purpose of this figure is to show the relation between deviation from normality of dis-

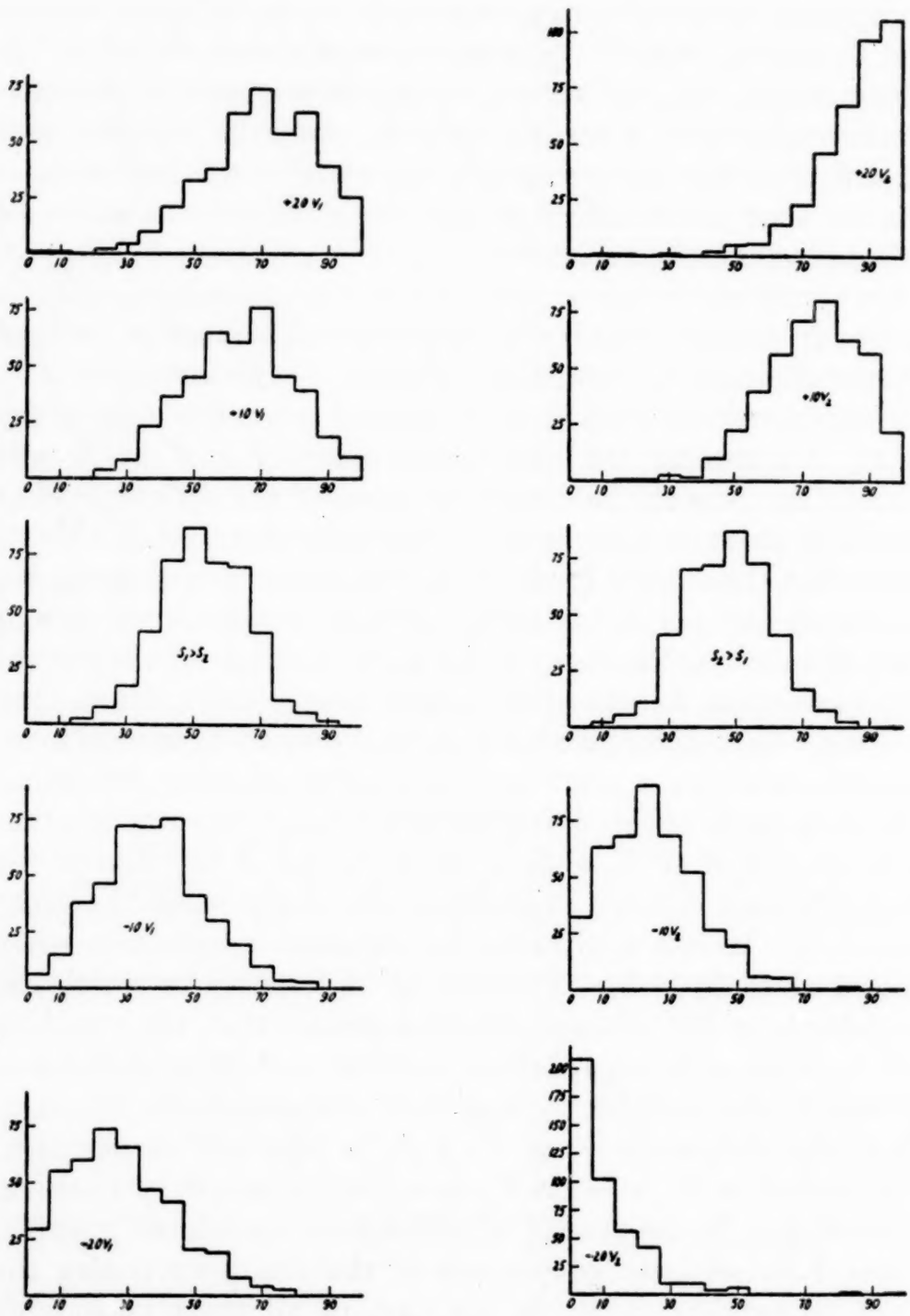

FIG. 1. Distribution of the population of 430 percent scores made under each of the comparison-situations. The abscissæ represent the percent scores, and the ordinates, their frequency.

tribution and the mean percent score. The greater the deviation from the mean, the greater the skew. As the mean approaches 50, however, the distribution approaches normal. It appears certain, that, had there been a distribution with a mean exactly at the 50 percent score, the scatter of the scores about the mean would have been very nearly symmetrical and the distribution would have rather closely approximated a normal one.

The data of Table I have been subjected to two quite different methods of absolute scaling. These may be termed the population percent and the score percent methods. The results obtained by the population percent method will be described first. This method represents a new application of a scaling formula derived by Thurstone [4] for determining the differences in ability between two or more age groups. It has already been extensively used by the writer for measuring the changes in difficulty produced by variation in the conditions of a given task.[5] The results given in Table I could also, no doubt, be expressed in terms of difficulty. The fundamental principle would be that the difficulty of perceiving a difference which on the average is correctly observed a very large percentage of the trials is very small, while the same difference with reverse sign is one which presents very great difficulty. Maximum, or infinite, difficulty would attach to the performance of observing a difference universally correctly reported, but to observe this difference always with the opposite sign, for example, to observe (or conscientiously to report), in 100 percent of the trials, as longer than the standard a variable reported by all other subjects in 100 percent of the trials as shorter than the standard.

The application of the concept of difficulty to the present data, while necessary for the answering of certain problems,

[4] A method of scaling psychological and educational tests. *J. Educ. Psychol.*, 1925, **16**, 440.

[5] See, for example, The measurement of difficulty, *Psychol Review*, 1936, **43**, 341–365; The interrelationship of conditions of difficulty: I. The effect of change in number at various spatial separations on simultaneous letter span, and II. Number, spatial separation and illumination as conditions of simultaneous letter span, *J. General Psychol.*, (in press).

is somewhat forced and will not be made in the present paper. The present problems are more simply formulated in terms of the magnitude of difference, as follows:

(1) What is the absolute magnitude of the effective difference produced by each of the various comparison-situations?

(2) What is the variability of each of these effective differences?

The sole assumption necessary to answer these questions by the population percent scaling method is that the magnitudes of the population of effective differences produced by any comparison-situation are normally distributed. As will be pointed out, the substantial validity of this assumption may be proved.

The steps to be taken in the scaling are as follows:

A. The proportions of the population making each score are changed into sigma values, on the normal distribution hypothesis. These values may be termed the population x-values. The transformation of Table I thus effected is presented as Table II.

TABLE II

POPULATION x-VALUES FOR THE VARIOUS PERCENT SCORES

Scores	$-20\ V_1$	$-20\ V_2$	$-10\ V_1$	$-10\ V_2$	$S_2>S_1$	$S_1>S_2$	$+10\ V_1$	$+10\ V_2$	$+20\ V_1$	$+20\ V_2$
96.7							−2.014	−2.576	−1.563	−1.067
90.0		−2.878				−2.878	−1.514	−1.589	−1.032	− .279
83.3		−2.878	−2.512		−3.090	−2.197	−1.011	− .885	− .527	+ .292
76.7	−2.512	−2.512	−2.226	−2.878	−2.366	−1.896	− .637	− .426	− .192	+ .745
70.0	−2.170	−2.457	−1.812	−2.878	−1.881	−1.265	− .154	+ .055	+ .246	+1.175
63.3	−1.825	−2.457	−1.412	−2.878	−1.412	− .646	+ .199	+ .490	+ .653	+1.506
56.7	−1.598	−2.457	−1.041	−2.197	− .860	− .215	+ .613	+ .915	+ .958	+1.825
50.0	−1.270	−2.457	− .693	−1.927	− .313	+ .313	+ .974	+1.359	+1.329	+2.034
43.3	− .849	−2.014	− .204	−1.468	+ .215	+ .860	+1.405	+1.812	+1.717	+2.366
36.7	− .507	−1.881	+ .215	−1.122	+ .646	+1.412	+1.927	+2.257	+2.054	+2.576
30.0	− .105	−1.626	+ .681	− .668	+1.265	+1.881	+2.409	+2.409	+2.366	+2.576
23.3	+ .329	−1.447	+1.080	− .228	+1.896	+2.366		+2.878	+2.576	+2.878
16.7	+ .736	−1.041	+1.635	+ .308	+2.197	+3.090		+3.090	+2.576	+2.878
10.0	+1.265	− .583	+2.144	+ .769	+2.878				+2.878	+3.090
3.3	+1.812	+ .043	+2.878	+1.447						

B. The relative variability of any two distributions is calculated by the formula, $\sigma_1/\sigma_2 = s_2/s_1$, in which s_1 and s_2 represent the values $(\Sigma x^2/n - m^2)^{\frac{1}{2}}$, calculated in the case of both distributions from the population x-values for the same set of scores. By m is meant the algebraic mean of the x-values of the particular scores used in calculating the s's.

C. The absolute difference between the mean of any two distributions (here the difference in the mean magnitude of the effective differences produced by any two comparison-situations) is calculated by the formula,

$$M_1 - M_2 = \left(m_1 - m_2\frac{\sigma_1}{\sigma_2}\right)\sigma_1$$

in which M represents the mean magnitude of an effective difference and m has the same significance as indicated above. The value of σ_1, which is the standard deviation of the distribution having the mean M_1, which may be the mean of any one of the distributions, is taken as the unit for measuring all other values. The unit which has been chosen is the standard deviation of the distribution of the differences in the case S_1 and S_2.

E. The difference of zero mean magnitude is derived from the consideration that the difference $S_2 > S_1$ has the same magnitude but opposite sign as the difference $S_1 > S_2$. The absolute zero therefore lies at the middle of the distance between these two differences. It may be defined as the effective difference which results in a number of 'longer' reports equal to the number of 'shorter' ones, or, when the 'equal' reports are divided evenly between these two categories, as in the present study, results in 50 percent of 'longer' reports.

One condition of the validity of this method is that the relation between the x-values for any two population distributions shall be linear and the coefficient of correlation + 1.0. Since the x-values are calculated on the basis of a normal distribution, the satisfaction of this condition constitutes proof that the distributions are normal, that is, proof of the validity of the sole required assumption. So far as the writer is aware, this is the only proof of the existence of normal distributions yet devised for these cases in which the base-line cannot be marked off in directly measured or known units. Now it is obvious from the distribution of the scores as regards their percent value, shown in Fig. 1, that the distributions are irregular. They cannot, then, be perfectly normal. The

most that may be expected is that they may be sufficiently close to normal to render the values obtained on the assumption of normality approximately correct. That such is the case appears to be indicated by the Pearson coefficients of correlation between the x-values of the columns of Table II. These coefficients, for pairs of adjacent columns when the columns are arranged in the same order as in Table II, are as follows: .963; .963; .994; .989; .997; .959; .990; .997; .988. With product-moment correlations so near to unity, it is hardly necessary to make any further tests for linearity. Clearly no very large error will result from the assumption that the distributions are normal; at least the errors will not be substantially greater than those due to the mere irregularity of the obtained data. In addition to this evidence that the distributions are approximately normal is the evidence, already mentioned, which consists in the fact that as the distributions approach a mean of 50 percent, they tend towards normal with the base-line marked off in percent scores.

The variability of all the distributions has been calculated in terms of that of the distribution of the differences S_1 and S_2. This calculation cannot be made directly, however, because a number of the distributions do not overlap the S_1 and S_2 distribution except at the unreliable, extreme ends. Only x-values between the limits, plus and minus 2 σ's, have been used. Consequently, a systematic 'dove-tail' procedure was followed, one by which, after the ratios σ_3/σ_2 and σ_2/σ_1 were determined, the ratio σ_3/σ_1 was calculated by the formula $\sigma_3/\sigma_1 = (\sigma_3/\sigma_2) \times (\sigma_2/\sigma_1)$. The σ of the S_1 and S_2 differences, used as the unit, or σ_1, is the same for $S_2 > S_1$ and $S_1 > S_2$; so that the column of proportions for either of these differences may be used in the calculation of the ratios σ_2/σ_1. The σ's of each of the distributions for situations involving one of the 20 percent variables (columns $-20\ V_1$, $-20\ V_2$, $+20\ V_1$, and $+20\ V_2$) were calculated in terms of the distributions for situations involving the 10 percent variable of the same sign and order. The σ's of the distributions involving one of the 10 percent variables, in turn, were calculated by comparison with whichever of the distributions $S_1 > S_2$ or $S_2 > S_1$ gave

the largest number of corresponding x-values within the used range of from plus to minus 2 σ's. One exception to this regular procedure was in the case of the $-10\ V_2$ column. Neither the columns $S_2 > S_1$ or $S_1 > S_2$ show more than five values, within the used range, which overlap those of the $-10\ V_2$ column. The standard deviation of the $-10\ V_2$ and S_1 difference was therefore first calculated in relation to the σ of the $-20\ V_1$ and S_2 difference. Eight pairs of values were available for that comparison. The exact calculations and the resulting relative standard deviations are indicated by the following table (Table III).

TABLE III

VALUES USED IN CALCULATING RELATIVE STANDARD DEVIATIONS OF THE DISTRIBUTION OF THE POPULATIONS OF DIFFERENCES

Differences	No. Usable x-values	m	$\sqrt{(\Sigma x^2/n) - m^2}$	Relative σ's
$-20\ V_1$ and S_2	8	−.636	.859	1.160
$-10\ V_1$ and S_2	8	+.033	.996	to 1.000
$-10\ V_1$ and S_2	8	−.398	.955	1.283
$S_2 > S_1$	8	−.056	1.225	to 1.000
$S_2 > S_1$	7	−.334	1.042	1.000
$S_1 > S_2$	7	+.334	1.042	to 1.000
$+10\ V_1$ and S_2	7	+.618	.829	1.305
$S_1 > S_2$	7	−.205	1.082	to 1.000
$+20\ V_1$ and S_2	8	+.394	.884	1.064
$+10\ V_1$ and S_2	8	−.016	.941	to 1.000
$-20\ V_2$ and S_1	6	−1.089	.657	1.318
$-10\ V_2$ and S_1	6	+.084	.866	to 1.000
$-10\ V_2$ and S_1	8	−.361	1.082	.918
$-20\ V_1$ and S_2	8	+.176	.993	to 1.000
$+10\ V_2$ and S_1	6	+.701	.758	1.222
$S_1 > S_2$	6	−.475	.926	to 1.000
$+20\ V_2$ and S_1	6	+.877	.717	1.171
$+10\ V_2$ and S_1	6	−.240	.839	to 1.000

The values given in Table III permit the calculation of the standard deviation of any population of differences in terms of the standard deviation of the distribution of the S_1 and S_2

differences. For example, the standard deviation of the $-20\ V_1$ and S_2 differences is 1.16 times that of the $-10\ V_1$ and S_2 differences. The σ of the latter, in turn is, 1.283 times that of the unit, namely, the σ of the S_1 and S_2 differences. The σ for $-20\ V_1$ and S_2 is therefore 1.16×1.283 units, or 1.488. Again the σ of the $-10\ V_2$ and S_1 differences is .918 times that of the $-20\ V_1$ and S_2 differences; it is therefore $.918 \times 1.488$, or 1.366 units. The values finally obtained, in terms of the σ of the distribution of the S_1 and S_2 differences, are shown in Table IV.

TABLE IV

RELATIVE STANDARD DEVIATIONS OF THE DISTRIBUTION OF DIFFERENCES UNDER VARIOUS COMPARISON-SITUATIONS

Comparison-Situation	σ, in Terms of $\sigma_{(S_1 \text{ and } S_2)}$
$-20\ V_1$ and S_2	1.488
$-10\ V_1$ and S_2	1.283
S_1 and S_2	1.000
$+10\ V_1$ and S_2	1.305
$+20\ V_1$ and S_2	1.389
$-20\ V_2$ and S_1	1.800
$-10\ V_2$ and S_1	1.366
S_1 and S_2	1.000
$+10\ V_2$ and S_1	1.222
$+20\ V_2$ and S_1	1.431

By the aid of the standard deviations given in Table IV and the means given in Table III (column m), the difference between the mean magnitudes of the various effective differences may be determined, by use of the equation given above (C) for the difference $M_1 - M_2$. To reduce the differences to units of $\sigma_{S_1 \text{ and } S_2}$, each mean given in Table III must be multiplied by the σ of its distribution.

The following table, Table V, shows the computations. The magnitudes yielded by these computations are the absolute mean magnitudes of the effective differences produced by the various comparison-situations. As already explained, the *sign* of the differences is arbitrary, but the data have been treated so that this sign appears as positive when the reports indicate the variable to be longer than the standard in over half the trials and negative when the reports indicate the

TABLE V

THE MEAN ABSOLUTE MAGNITUDE OF THE EFFECTIVE DIFFERENCES UNDER THE VARIOUS COMPARISON-SITUATIONS

A. Computation Equations
(Differences between differences)

$(-20\ V_1 \text{ and } S_2) < (-10\ V_1 \text{ and } S_2) = (-.636 \times 1.488) - (+.033 \times 1.283)$	$= -\ .988$
$(-10\ V_1 \text{ and } S_2) < (S_2 > S_1) = (-.398 \times 1.283) - (-.056)$	$= -\ .455$
$(+10\ V_1 \text{ and } S_2) > (S_1 > S_2) = (+.618 \times 1.305) - (-.205)$	$= +1.012$
$(+20\ V_1 \text{ and } S_2) > (+10\ V_1 \text{ and } S_2) = (+.394 \times 1.389) - (-.016 \times 1.305)$	$= +\ .568$
$(S_1 > S_2) > (S_2 > S_1) = (+.334 \times 1.0) - (-.334 \times 1.0)$	$= +\ .668$
$(-20\ V_2 \text{ and } S_1) < (-10\ V_2 \text{ and } S_1) = (-1.089 \times 1.800) - (+.084 \times 1.222)$	$= -2.063$
$(-10\ V_2 \text{ and } S_1) < (-20\ V_1 \text{ and } S_2) = (-.361 \times 1.366) - (+.176 \times 1.488)$	$= -\ .755$
$(+10\ V_2 \text{ and } S_1) > (S_1 > S_2) = (+.701 \times 1.222) - (-.475)$	$= +1.332$
$(+20\ V_2 \text{ and } S_1) > (+10\ V_2 \text{ and } S_1) = (+.877 \times 1.431) - (-.240 \times 1.222)$	$= +1.548$

B. Absolute Magnitudes
(Difference of each difference from zero)

Effective Difference	Absolute Magnitude
$-20\ V_1 \text{ and } S_2 = -\ .789 - .988$	$= -1.777$
$-10\ V_1 \text{ and } S_2 = -\ .334 - .455$	$= -\ .789$
$S_1 > S_2 = +\ .334$	$= +\ .334$
$+10\ V_1 \text{ and } S_2 = +\ .334 + 1.012$	$= +1.346$
$+20\ V_1 \text{ and } S_2 = +1.346 + .568$	$= +1.914$
$-20\ V_2 > S_1 = -2.532 - 2.063$	$= -4.595$
$-10\ V_2 > S_1 = -\ .755 - 1.777$	$= -2.532$
$S_2 > S_1 = -\ .334$	$= -\ .334$
$+10\ V_2 \text{ and } S_1 = +\ .334 + 1.332$	$= +1.666$
$+20\ V_2 \text{ and } S_1 = +1.666 + 1.548$	$= +3.214$

variable to be shorter than the standard in over half of the trials. Further, the difference $S_1 > S_2$ is taken as positive, and therefore the difference $S_2 > S_1$ as negative. Obviously, since the latter differences, the S_1 and S_2 differences, are of the same magnitude, but of different sign, each differs from the absolute zero magnitude of difference by one-half their difference from each other. The line reading $(S_1 > S_2) > (S_2 > S_1)$ means, then, that the positive difference $(S_1 > S_2)$ is algebraically greater than the negative difference $(S_2 > S_1)$ by + .668 times the σ of either of these differences—either of them, because they have the same σ. Measured from the absolute zero, either difference has the magnitude, neglecting sign, of .334 times $\sigma_{(S_1 \text{ and } S_2)}$.

In the score percent scaling method, the sole population considered in the case of a given comparison-situation is that made up of all the reports obtained under that situation. There is only one score in the case of each comparison-situation, namely, that derived from the entire 6,450 reports (or, in the case of S_1 and S_2, 12,900 reports). The reports must be scored as of one of two categories (by dividing the equal judgments between 'longer' and 'shorter'). The scaling is then accomplished on the assumption that the percent scores are related to the mean magnitude of the effective difference as the ogive of a normal distribution curve to its base-line. On this assumption, all that is necessary in order to obtain the absolute effective difference represented by any percent score is to note its σ value. Thus a percent score of 25 indicates an effective difference of $-$ 1 P.E.; one of 50, an effective magnitude of zero; and one of 84.1, a magnitude of $+$ 1.0, etc. The *unit*, by this method, is a much larger one than by the population percent scaling method; but a change in the unit should have no effect upon the relative magnitude of the measured differences. The percentages of all judgments indicating the variable to be longer than the standard, and the σ values of these percentages are shown in Table VI. For the sake of completeness, the total number of 'equal' judgments is also included in the table. The percent scores given in Table VI are shown graphically in Fig. 2.

In order to compare the results obtained by the two scaling methods, the measures of the magnitude of the various effective differences must be reduced to a common unit. A simple method of doing this is to consider the entire range of differences to be represented by a scale of 100 points. Any difference may be converted into units of this scale by multiplying by 100 and then dividing by the total range of the differences obtained by the same method as that used in calculating the difference in question. For example, in the case of the score percent scaling method, the differences extend from $-$ 1.419 to $+$ .954 or over a range of 2.373. All the absolute values given in Table VI are therefore multiplied by 100/2.373. Similarly, the absolute values in Table V may

TABLE VI

ABSOLUTE MAGNITUDE OF EFFECTIVE DIFFERENCES SCALED BY THE SCORE PERCENT METHOD

Each percent score shows the percentage of all trials in which the duration mentioned first in the column headed 'Comparison-Situation' was judged longer than the duration mentioned second.

The nos. in the case of S_1 and S_2 have been divided by 2 to make them comparable with those for the other situations.

Comparison-Situation	No. of Reports			Percent Score	Absolute (σ) Value
	Longer	Shorter	=		
-20 V_1 and S_2	1,714	4,358	378	29.5	− .539
-10 V_1 and S_2	2,359	3,588	503	40.5	− .240
$S_1 > S_2$	3,227	2,675	548	54.3	+ .108
$+10$ V_1 and S_2	4,034	1,902	514	66.5	+ .426
$+20$ V_1 and S_2	4,487	1,569	394	72.6	+ .601
-20 V_2 and S_1	425	5,872	153	7.8	−1.419
-10 V_2 and S_1	1,199	4,872	379	21.5	− .789
$S_2 > S_1$	2,675	3,227	548	45.7	− .108
$+10$ V_2 and S_1	4,278	1,664	508	70.3	+ .533
$+20$ V_2 and S_1	5,170	917	363	83.0	+ .954

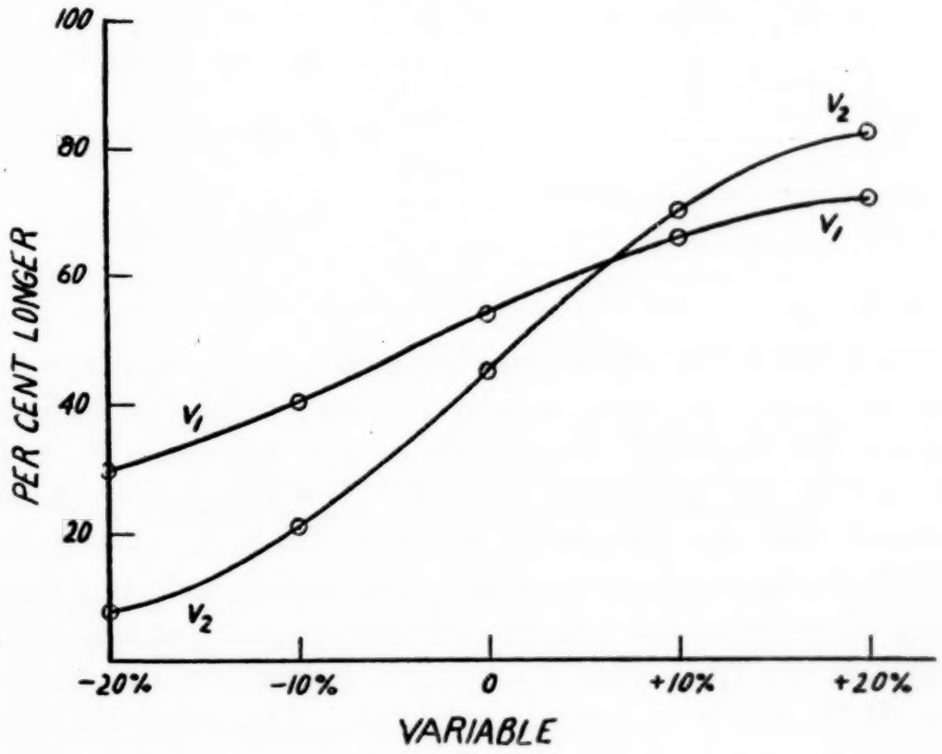

FIG. 2. Percent scores of total group (ordinates) in case of each of the variables (abscissæ).

be multiplied by 100/7.809. The results are shown in Table VII.

The results given in Table VII may be represented by a diagram, showing on two parallel lines the magnitude of the various effective differences as determined by the two methods of scaling. Both scales have an absolute zero point, that is, a point representing a mean difference of zero magnitude. In both scales, zero effective difference may be defined as the difference occurring upon comparison of two physically identical stimuli, when the effect of order of presentation is eliminated. The mean effective difference between two identical stimuli may be regarded as wholly the effect of order;

TABLE VII

COMPARISON OF ABSOLUTE MAGNITUDES OF THE VARIOUS EFFECTIVE DIFFERENCES DETERMINED BY TWO METHODS, ON SCALE OF 100 STEPS

Effective Difference	Magnitude	
	Population Percent Method	Score Percent Method
-20 $V_1 > S_2$	-22.8	-22.7
-10 $V_1 > S_2$	-10.1	-10.1
$S_1 > S_2$	$+4.3$	$+4.6$
$+10$ $V_1 > S_2$	$+17.2$	$+18.0$
$+20$ $V_1 > S_2$	$+24.5$	$+25.3$
-20 $V_2 > S_1$	-58.8	-59.8
-10 $V_2 > S_1$	-32.4	-33.2
$S_2 > S_1$	-4.3	-4.6
$+10$ $V_2 > S_1$	$+21.3$	$+22.5$
$+20$ $V_2 > S_1$	$+41.2$	$+40.2$

therefore, with the effect of order eliminated, the difference must be zero. In other words, the zero point lies halfway between the algebraic magnitude of the difference $S_1 > S_2$ and that of the difference $S_2 > S_1$. Incidentally, it should be noted that the differences given in Table V, part *B*, are the differences between two differences, namely, the one listed in the column headed effective difference and one not listed at all, namely, the difference of zero magnitude, *i.e.*, S_1 and S_2 with the effect of order eliminated. The presence of an absolute zero in both scales makes it possible to align them: so that if the scales are placed vertically, the zero of one lies at the same height as the zero of the other.

Simply for the sake of distinctness, both scales are pictured twice in Fig. 2, so that the differences obtained with the variable first are shown separately from those obtained with the variable second. As hardly needs to be stated, the whole diagram is drawn on the same scale, so that, if desired, the differences with variable first may be projected onto the scale showing the differences with variable second.

The amazingly close agreement between the results obtained by the two methods is obvious, either from Table VII or from Fig. 3. Since the sole assumption made in the

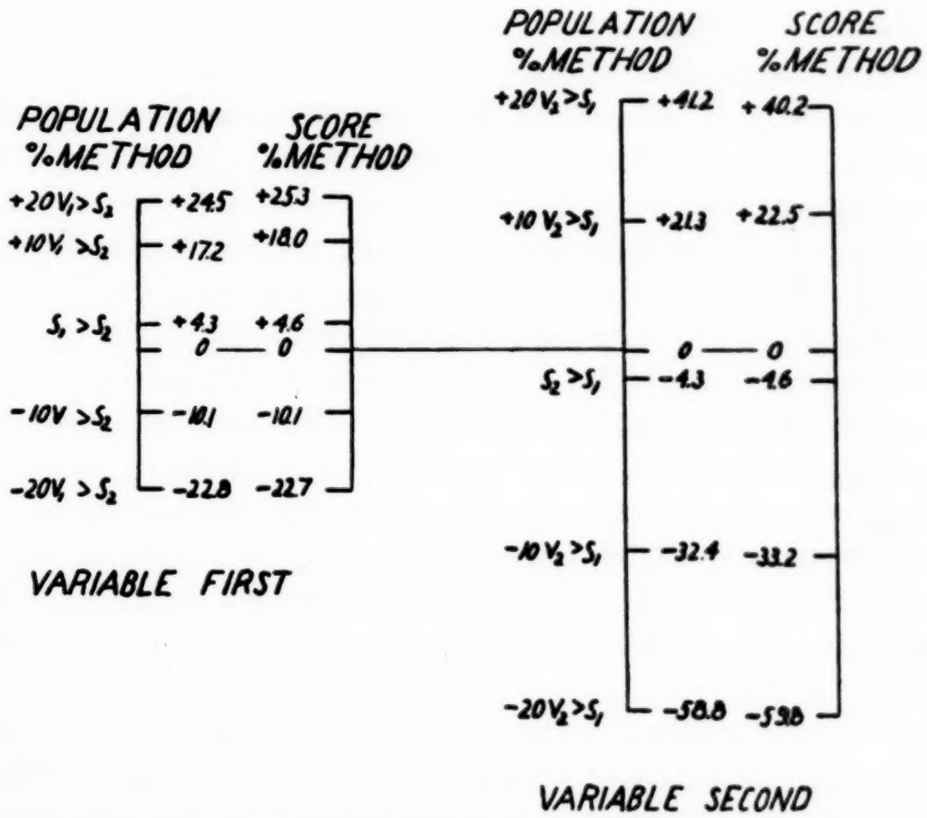

FIG. 3. Representation on a scale of 100 equal steps of the absolute magnitude of each effective difference, as determined by two scaling methods.

population percent scaling method affords its own internal check, the conclusion may be drawn that both methods are equally valid. The assumptions of the two methods, as well as the computations, are quite distinct. The reason for the agreement in outcome is obvious. It lies simply in the fact that in order to change the skewed distributions shown in

Table I and Fig. 1 into the assumed normal ones, it is necessary progressively to stretch the percent scores apart as they approach, from 50, either 0 or 100. It turns out that the necessary stretching results in the percent scores finally being spaced so that they agree with the assumption of the score percent scaling method: the scaled position of any score is the point on the base-line of a normal distribution below which, *i.e.*, to the left of which, lies a percentage of the total distribution-area equal to the percentage which constitutes the score.

The score percent method does not lend itself so neatly to the calculation of variability as does the population percent method. However, since the two methods yield practically the same result as regards the absolute magnitude of the various effective differences, it seems worth while to attempt also to obtain measures of variability by both methods. Estimates by the score percent method may be obtained in three ways, depending upon which of several variabilities is desired. Results obtained by all three methods are listed in Table VIII. The simplest method is to scale the percent scores of each subject. There are thus obtained as many measures of the magnitude of each difference as there are subjects, namely 43. The standard deviation of these 43 scaled magnitudes may then be directly calculated in the usual manner. These standard deviations, each of which shows the variability from subject to subject in an effective difference, are shown in column I of Table VIII (column headed '43 S's').

A second method is to calculate the variability of the differences in the case of a single subject from sitting to sitting. Since the percent scores of a single subject in the case of any one difference are based on as few as 15 trials, these percent scores are very unreliable. Moreover, their σ is the standard deviation of only 10 measures (since there were only 10 sittings). Still more serious is the fact that while no subject obtained a percent score of either zero or 100 for the entire 10 sittings, yet many of the daily percent scores, based on 15 trials, were either zero or 100, and therefore not scalable. In spite of these difficulties, and the necessarily unreliable re-

sults, the method has been used, substituting a score of 0.25 or 99.75 in place of scores of zero or 100 percent, respectively. After the standard deviation for each individual was calculated, the average for all individuals was taken, and it is this average which is used in column II of Table VIII (column headed 'Av. Individ.'). These values constitute an estimate of the standard deviation of the mean magnitudes of any difference in the case of the average individual, each mean magnitude being that indicated by the reports of a single subject at a single sitting.

The third method, and the one which should give results more nearly in harmony with those obtained by the population percent method, is to scale the entire population of 430 percent scores obtained for each difference, and to calculate the standard deviation of the resulting magnitudes. The difficulty of zero and 100 percent scores still exists. To obviate this difficulty the semi-interquartile range has been used instead of the standard deviation. The semi-interquartile range is not affected by scores of 0 or 100 unless these constitute over one-fourth of the population of scores. In the one case where such scores did constitute over one-fourth the total number, namely, -20 V_2 and S_1, no valid determination of the S. I. Q. R. was possible. As the best possible estimate under such circumstances, note has been made of the difference between the median and the 1st quartile. The relative magnitude of these S. I. Q. R. values is shown in the column of Table VIII headed '430 Scores.'

In the tabulation of the different variabilities (Table VIII), each variability has been expressed as a ratio of the variability of the difference S_1 and S_2 (calculated, of course, by the same method).

Whatever be the degree of unreliability which may attach to the various methods of computing variability, the results certainly are adequate to establish beyond question a very significant general trend, namely, the increase in variability with the magnitude of the difference. In the case of either time-order, V_1 or V_2, the variability, no matter how calculated, shows a minimum under the situation S_1 and S_2; and then,

TABLE VIII

VARIABILITY IN THE MAGNITUDE OF THE VARIOUS EFFECTIVE DIFFERENCES IN UNITS OF THE VARIABILITY OF THE EFFECTIVE DIFFERENCE S_1 AND S_2

Comparison-Situation	Score Percent Method			Population Percent Method
	43 S's	Av. Individ.	430 Scores	430 Scores
$-20\ V_1$ and S_2	1.61	1.46	1.46	1.49
$-10\ V_1$ and S_2	1.14	1.37	1.11	1.28
S_1 and S_2	1.00	1.00	1.00	1.00
$+10\ V_1$ and S_2	1.43	1.46	1.32	1.31
$+20\ V_1$ and S_2	1.82	1.49	1.57	1.43
$-20\ V_2$ and S_1	2.44	1.67	2.46	1.80
$-10\ V_2$ and S_1	1.58	1.55	1.49	1.37
S_2 and S_1	1.00	1.00	1.00	1.00
$+10\ V_2$ and S_1	1.24	1.38	1.34	1.39
$+20\ V_2$ and S_1	1.40	1.61	1.67	1.43

without exception, it increases as the deviation of the variable from the standard increases, irrespective of whether the deviation is positive or negative. With respect either to the physical or the effective differences, it may be stated that the variability increases as the difference increases. It is apparent, however, that variability is more closely related to the effective, than to the physical, differences. This is shown with particular clearness when the comparison-situation includes a variable shorter than the standard, since in this case the lack of identity between physical and effective differences is especially striking. For example, the difference $-20\ V_1$ and S_2 is physically the same as the difference $-20\ V_2$ and S_1; but, as apparent from Fig. 3, the effective difference produced by $-20\ V_2$ and S_1 is more than 2.5 times as great as that resulting from $-20\ V_1$ and S_2. The *variability* is likewise decidedly greater in the case of $-20\ V_2$ and S_1. Thus, where the physical differences are equal but the effective differences unequal, the variability tends to follow the effective difference. The same relationship holds in the case of the situations $-10\ V_2$ and S_1 and $-10\ V_1$ and S_2. Both the mean magnitude and the variability are decidedly greater in the case of $-10\ V_2$ and S_1. Again, $+20\ V_2$ and S_1 represents a physical difference identical with $-20\ V_2$ and S_1; but the effective difference is much greater in the latter case;

so also is the variability. This closer relationship of variability to effective than to physical difference is presumably what should be expected, since it is the variability of the effective difference that has been measured. The significant conclusion, then, is that variability varies with the magnitude of the effective difference, irrespective of whether the difference is positive or negative. The coefficient of linear correlation, *r*, uncorrected for attenuation, between the ten effective magnitudes and their variabilities, is + .89 (using results obtained by the population percent scoring method). The magnitude of the effective difference appears to determine the degree of variability; and with this magnitude kept constant, the partial correlation between variability and time-order does not appear to be significant. Likewise, it is not clear that it greatly matters, as far as variability is concerned, whether the effective difference is negative or positive.

It is probably not worth while, on account of the paucity and unreliability of the determinations of the variabilities, to attempt to formulate in a precise quantitative law the relation between variability and magnitude of effective difference. The standard deviations obtained by the population percent scaling method (see Table IV) have, however, been plotted against the corresponding effective differences obtained by the same method (listed in Table V). This plot constitutes Fig. 4. The figure shows the striking increase in variability with increase in either the negative or the positive magnitude of the effective difference. Whether the relationship is linear or negatively accelerated, or even positively accelerated, cannot be reliably determined from the obtained data. The curve drawn in Fig. 4 suggests a negatively accelerated relationship, one very likely involving either a square root or a logarithmic function. This curve, however, is a free hand, speculative curve, and is drawn merely to suggest an unproven possibility. The one fact clearly proved is the increase in variability with the magnitude of the effective difference.

These findings concerning the variability of the effective differences are quite in contrast with those which might be expected were the effective differences conceived as produced

by two independently acting physical stimuli, namely, the standard and the variable. On this latter hypothesis the $\sigma_{diff.}$ would equal $(\sigma_s^2 + \sigma_v^2)^{\frac{1}{2}}$, in which σ_v represents the standard deviation in the effect of the variable and σ_s represents the standard deviation in the effect of the standard (always present as one of the pair of compared durations). Since the standard remains the same throughout, σ_s would remain constant. With the unit of measurement chosen so that σ_s would equal unity, and with σ_v measured in terms of the same unit, the formula could be written $\sigma_{diff.} = (1 + \sigma_v^2)^{\frac{1}{2}}$.

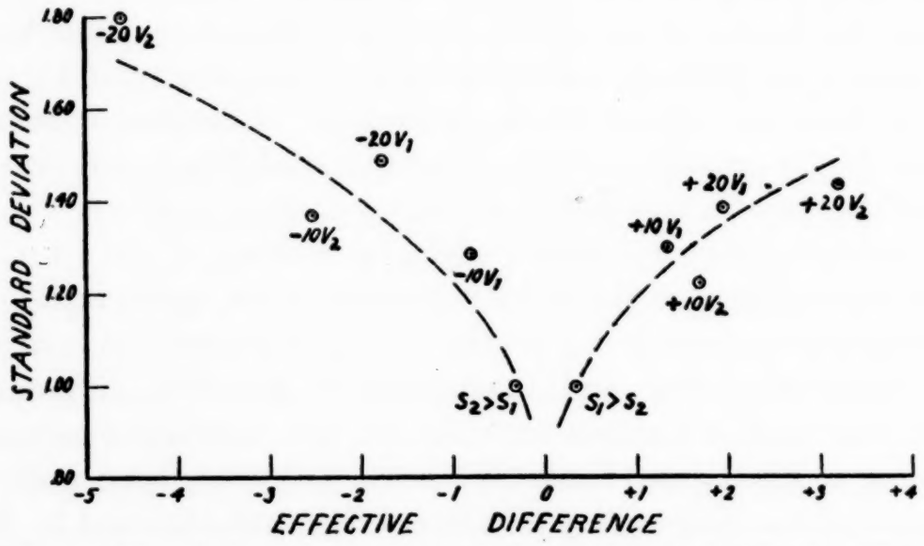

FIG. 4. The relation between the standard deviations of the effective differences (ordinates) and the algebraic magnitudes of those differences (abscissæ). Both the differences and the standard deviations are measured in terms of the unit consisting of the standard deviation of the effective difference S_1 and S_2.

How σ_v would vary with the magnitude of the variable, on the hypothesis that the variable exerted its effect independently of the standard, cannot be definitely stated. This σ could not, however, by any one with faith in the regularity of natural processes, be conceived as varying with magnitude the way it would have to vary in order to bring the obtained results, shown in Fig. 4, into harmony with the hypothesis that $\sigma_{diff.} = (1 + \sigma_v^2)^{\frac{1}{2}}$. Such harmony could be established only by supposing that σ_v at first decreased as the magnitude of V increased, and then, suddenly, began to increase as the magnitude of V increased still further. In other words, the

variability of the hypothetical, independently acting variable would have to show, when plotted against the magnitude of the variable, the peculiar shape of the curve in Fig. 4.

Perhaps a less improbable, though purely hypothetical explanation could be reached by first completing the formula for the $\sigma_{diff.}$ by assuming a correlation in the effects of the two compared stimuli, that is, by using the formula, $\sigma_{diff.} = (\sigma_s^2 + \sigma_v^2 - 2r_{sv}\sigma_s\sigma_v)^{\frac{1}{2}}$. This formula assumes that when each effect of *S* is paired with the effect of the *V* used at the same trial, the two effects will show a correlation. It might then be assumed that the σ_v remained constant at all magnitudes and, further, that there was a positive correlation between *S* and *V* and that the amount of this correlation decreased with the magnitude of the effective difference between *S* and *V*, irrespective of the direction of the difference. On such assumptions, the obtained results could be explained. Such an explanation of course admits that the compared stimuli do not act as though independently presented, for then their effects would be uncorrelated.

A simpler way of looking at the matter is to recognize that what we have been calling a difference consists in but one unitary event, namely, a change from one internal condition to a second condition (the second internal condition being the one instigated by the second member of the comparison-pair). From this point of view, the relationship here brought to light may be summed up in the very simple statement that the variability in the magnitude of the *change* produced by the second stimulus increases with the magnitude of that change, irrespective of its direction. The first condition, the one *from* which the change occurs, is the resultant of a number of factors. One of these is the immediate effect of the first member of the comparison-pair, but others are the factors determining the pre-existing condition affected by this first member, among which factors are the residual effects of the preceding portion of the whole comparison-series and those conditions which account for the existence of an indifference-duration, and, also, the effect of the lapse of time before the presentation of the second member of the comparison-pair.

If the two levels (the one, that in which the change begins, and the other, that in which it ends) be termed effective levels, then it is the variability of these two effective levels which would have to be referred to, if it were still desired to express the variability in terms of the formula for the standard deviation of a difference. But, the value of conceiving the variability of a change in terms of the variability of two levels seems rather doubtful. The second of these two levels, at least, has no existence apart from the change, and its variability depends upon the variability of the change, rather than vice versa. The case is not like that of the difference between two variable performances, for example, on the part of two individuals, the variability of whose scores has an existence independent of, and unaffected by, the magnitude of the difference between their means. In the present instance, it is precisely upon its magnitude that the variability of any effective difference depends.

The significance for psychophysics of the results here reported will be apparent to anyone familiar with an equation which has been advanced [6] as the fundamental psychophysical equation. That equation evaluates the magnitude of the effective difference between two stimuli in terms of the standard deviation of the difference, with the $\sigma_{diff.}$ regarded as computed from the standard deviation of the effects of the single stimuli taken separately. The equation is as follows: $S_k - S_a = X_{ka}(\sigma_k^2 + \sigma_a^2)^{\frac{1}{2}}$, in which $S_k - S_a$ is what has here been termed the mean effective difference (between the effects of the compared stimuli, R_k and R_a) and X_{ka} is the sigma value for the experimentally observed proportion of judgments 'R_k greater than R_a.' The values σ_k and σ_a are the standard deviations in the effect produced by the corresponding stimuli. The present study, while not necessarily proving any lack of validity on the part of this formula, considered merely as a description, does point to very definite limitations in its usefulness. For if the σ of the effect produced by one of the stimuli, the variable, depends upon its difference from the effect produced by the standard, the effective difference would

[6] L. L. Thurstone, Psychophysical analysis, *Amer. J. of Psychol.*, 1927, **38**, 380.

have to be already known before the formula designed to yield it could be used. But then there would be no need for the formula. And if the formula must first be completed by taking into consideration the correlation r_{ka} then the application of the formula would become impracticable. In the present work no attempt has been made to derive the σ of an effective difference from that of the σ's of the two compared effects taken separately. Such an attempt would apparently be futile. Instead, the value of $\sigma_{diff.}$ has been calculated directly as though the difference were a unitary affair and then the magnitude of the difference arising in any comparison-situation has been determined by calculating how much greater that difference is than a difference of zero (the difference which gives the value zero to X_{ka} in the above equation, *i.e.*, the difference which results in equal frequencies of the judgments of longer and of shorter).

Summary

By a group of 43 subjects, a total of 64,500 comparisons were made, in accordance with the method of constant stimuli, of a standard consisting of a tonal duration of .5 sec., and five variables differing from the standard by from − 20 to + 20 percent. Since both time-orders, that is, variable 1st and variable 2d, were used, there were 10 comparison-situations. The mean absolute magnitude of the differences perceived (termed effective differences) in each of these 10 comparison-situations was determined by two different methods of absolute scaling. In the same unit, the variability of each of these differences was also determined. The two scaling methods yielded nearly identical magnitudes for the various effective differences and showed the same general trend in the variabilities of these differences. The obtained results make it possible to picture graphically, in an equal unit scale, with an absolute zero, both the mean magnitude and the variability of each of the 10 effective differences.

When the standard deviations of the various effective differences are plotted against the *algebraic* magnitude of the differences, the curve (shown in Fig. 4) shows a fall in varia-

bility with decrease in the negative magnitude, and then a rise with increase in the positive magnitude, of the effective differences. This peculiar variability curve could not occur if the compared durations acted as independent, *i.e.*, separately presented, stimuli; for then, on any plausible assumption concerning the relation of variability to absolute magnitude, the formula for the standard deviation of a difference would not give the observed results. On the other hand, if each effective difference be conceived as a unitary event, namely, a *change* from one level to another, then the relation of variability to the magnitude of this change, irrespective of its direction, becomes a simple and understandable one: variability increases with increase in the magnitude of the change.

(Manuscript received January 27, 1936)

THE ROLE OF PAST EXPERIENCE IN THE VISUAL APPREHENSION OF MASKED FORMS [1]

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The present study [2] was suggested by K. Gottschaldt's investigation of the influence of experience upon the perception of figures.[3] He employed as material a series of five simple geometrical figures for impression (the *a*-figures) and thirty-one complex figures for test (the *b*-figures) in each of which an *a*-figure was concealed as an inconspicuous part. They were drawn with black ink upon white cards and were exposed by projection on the screen. To a first group of observers, three in all, he presented, with the request to learn and to name, each *a*-figure three times, followed by the series of *b*-figures. Exposure time for the former set of figures was 1 second, and for the latter, 2. With no other instruction than to see and describe verbally the *b*-figures, the observers were kept ignorant of the fact that the *a*-figures were present in the *b*-figures. A second group of eight observers went through the same procedure with the only exception that the number of repetitions of the *a*-figures was increased to 520. Thus the amount of experience was supposed to vary with the number of times an *a*-figure was shown by itself, and the effectiveness of experience was measured by the number of times that figure was noted in a complex pattern containing it.

[1] A dissertation submitted in partial fulfillment of the requirements for the degree of doctor of philosophy in the University of Michigan.

[2] The writer wishes to express her warm thanks to Professor John F. Shepard for his constant encouragement and untiring criticism on both the experimental work and the preparation of this report. Her thanks are also due to Professor W. B. Pillsbury for his kind advice and for making available the greater number of the subjects.

[3] Kurt Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren. I. Über den Einfluss gehäufter Einprägung von Figuren auf ihre Sichbarkeit in umfassenden Configurationen, *Psychol. Forsch.*, 1926, **8**, 261–317.

Gottschaldt found that in only 6.6 percent of the cases where the *a*-figures were learned with 3 repetitions, the *b*-figures were described in terms of having contained *a*-figures; whereas intensifying the impression by 520 repetitions of the *a*-figures yielded a result of 2.9 percent. These percentages included both cases in which the *a*-figures stood out immediately and cases in which the *b*-figures were seen and described as such and then the appearance of the *a*-figures occurred.

To find out how the addition of instruction to seek out the concealed *a*-figures would affect the results, Gottschaldt took the same two groups of observers and presented the *a*-figures twice more to the group who had seen them 3 times and 20 times more to the group with 520 original repetitions. The observers were given instruction to look for the five learned figures in the complex patterns. The results showed that the percentages of successful finding of the *a*-figures in the two experimental groups were 21.5 and 19.4 respectively.

Gottschaldt came to the conclusion that the number of times a simple figure was shown (3 or 520, 5 or 540) made no difference in the likelihood of its being seen in a complex one; that is, the effect of experience was slight in the face of the strong 'belonging together' of natural wholes. However, a considerable increase of positive cases was obtained by giving instructions to the observers to look for the simple figures. Even so, the degree of coherence in the larger figure was still a decidedly limiting factor, he said. It was concluded then that the effect of accumulated repetition of a part upon the perception of a greater complex was very small and that the influence of the inner test situation of the observer was of greater significance. This latter statement Gottschaldt greatly elaborated in a separate series of experiments.[4]

He used easy figures to study how a slight variation in the procedure affected perception. After an *a*-figure was exposed successively, for size-comparisons, 40 times, a *b*-figure was

[4] Kurt Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren. II. Vergleichende Untersuchungen über die Wirkung figuraler Einprägung und den Einfluss spezifischer Geschehensverlaufe auf die Auffassung optischer Komplexe, *Psychol. Forsch.*, 1929, 12, 1–87.

inserted between two *a*-figures to be compared as a disturbance picture, and the observers were instructed to describe the *b*-figure. It was found that if this instruction was made at the beginning of the experiment before the learning began, the *b*-figure was seen as the *a*-figure plus a line; but if the instruction came immediately after the learning series and before the triple exposure, the *b*-figure was seen as a whole with no relation to the *a*-figure.

Another group of experiments dealt with the question whether the appearance of *b*-figures from the threshold was influenced by previous learning of the *a*-figures. The results showed that the sequence of the phases of the appearing depended upon the course of experimental procedure; if the situation-vector directed the perception toward the *a*-figure, the *b*-figure came into being by starting with the *a*-figure. When the test-figure was inserted between the two comparison figures without any interpolated instruction, the *a*-figures were seen in the *b*-figures; but when the observer was informed at the end of the learning series and before the test that new figures were about to appear, the *a*-figures were not seen. Furthermore, in alternating two *a*-figures in learning, if *b*-exposures entered and fitted in the order of the series, the *a*-figure was perceived in the *b*-figure, but if they did not fit in, *e.g.*, if the entering of a *b*-figure was not directed toward its own *a*-figure, no trace of an after-effect of experience was found.

In a third group of experiments, the effect of the interval between learning and test was studied. It was found that in only 1.2 percent of the cases studied for the immediate effect of previous experience the comprehension of the *b*-figure was determined by the *a*-figure. However, if the situation of the *b*-exposure contained a vector directed toward the *a*-figure, as, for instance, when an *a*-figure was being described the exposure of the *b*-figure came to interrupt, the *a*-figure was seen in the *b*-figure. The same results were obtained when the *b*-figures in the test followed the rhythm used for the *a*-figures.

Therefore according to Gottschaldt, situation-vectors are the essential factors in perception. In his own words, "We

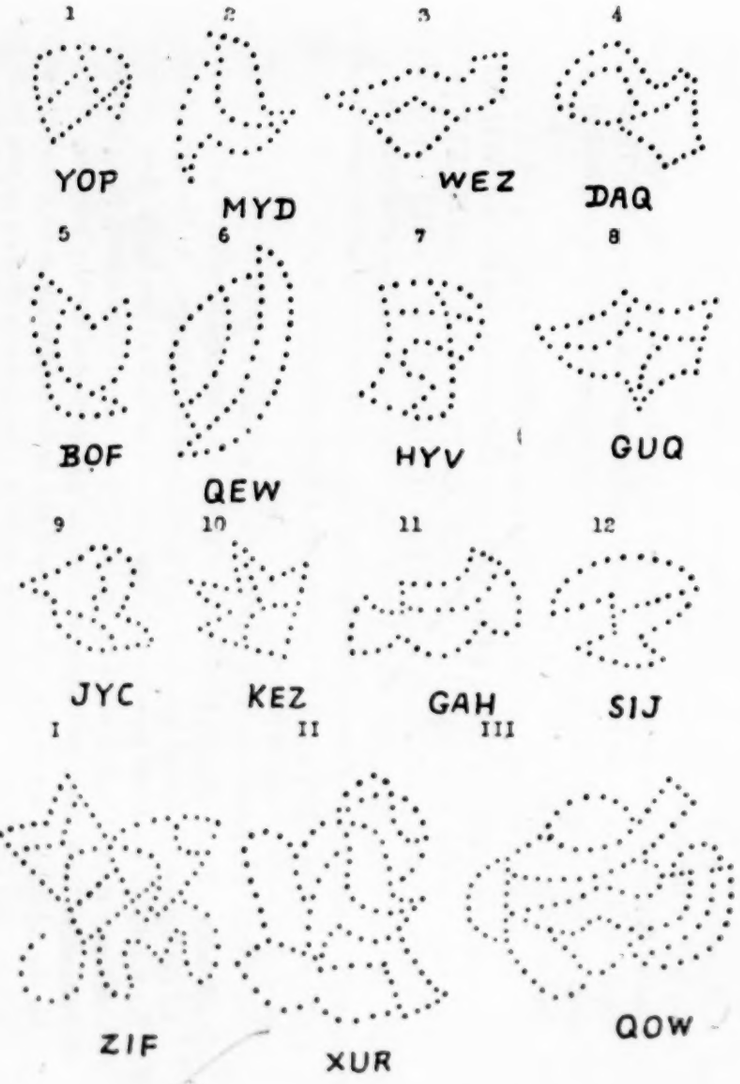
are justified to assume that all results of perception of *a*-figures in *b*-figures are to be traced back to the effect of the situation-vectors."

With many of Gottschaldt's figures, it is nearly or quite impossible to organize the complex figures so that the simple figures will appear as units of the complex figures. Clearly there are factors which sharply delimit the possibilities of organization, and numerous repetitions are practically without result in changing this limitation. We need not at this time consider what these limiting factors are. However, suppose we should design simple and complex figures such that, in addition to organization in which all or a characteristic part of the simple figure functions as a unit of the complex figure, many other organizations of the complex figure are more or less equally probable; suppose, furthermore, that the technique gave no situation-vector or set which might tend to isolate the simple in the complex; what then would be the effect of past experience? That was our problem.

A number of preliminary experiments were carried out which we need not describe in detail, since they only led to the technique used in the main investigation. They suggested that dotted figures lend themselves to more varied organization than those drawn in solid line. It proved impractical to use familiar objects, forms, etc. as the embedded figures, since one cannot then obtain a satisfactory control group of observers. It was shown that parts of the simple figures no more than the whole must occur as definitely isolated units in the complex figures, since it then becomes only a direct recall to pass from the definitely isolated part to the whole simple figure. In view of these findings, the following technique was designed to solve the problem as stated above.

Material and Procedure

The material employed in this experiment consisted of a series of 12 irregular figures composed of dotted lines. This might be called the simple-series. Another series of 12 figures of a more complex nature, each masking a figure of the simple-series, formed the masked-series. All these were lithographed on white paper, each mounted on a cardboard 5 × 3 in. Below each figure was a nonsense syllable, as shown in the accompanying page. On account of the nature of the procedure, to be described in the following paragraphs, many copies of the figures were provided. The figures are shown in the charts.


1
2
3
4
YOP
MYD
WEZ
DAQ
5
6
7
8
BOF
QEW
HYV
GUQ
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JYC
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ZIF
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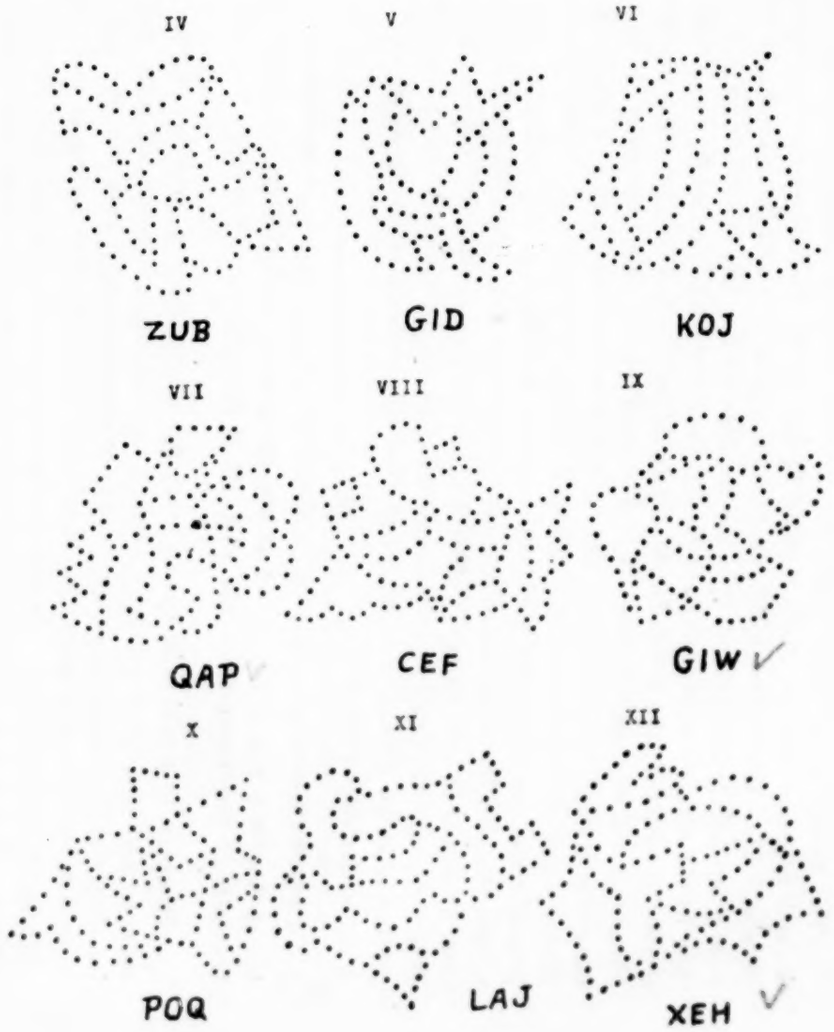
QOW


IV
V
VI
ZUB
GID
KOJ
VII
VIII
IX
QAP
CEF
GIW
X
XI
XII
POQ
LAJ
XEH

The figures in the simple-series were divided into two halves of six each—the A- and B-series—and the corresponding figures in the masked-series were treated likewise. The observers were separated into two groups, A and B, according as they were presented the simple-series A or the simple-series B. The masked-series A would then be the test-series for the Group A observers; the masked-series B, their control-series. Conversely, the masked-series B would be the test-series for the Group B observers; and the masked-series A, their control-series. By 'test-series' is meant the series in which those observers were tested as to their ability to pick out the particular simple-figure, now masked, as a unit separate from the rest. 'Control-series' is used here to mean the series in which the observers, having had no previous experience of the concealed figures, served as controls for those who did. Thus, all masked-figures were presented to both groups of observers, but the arrangement differed for the two. If we represent the figures of the simple-series by Arabic numerals and those of the masked-series by Roman numerals, we shall be able to demonstrate the distribution in a clearer fashon, as follows:

	Impression-series	*Test-series*	*Control-series*
Group A	1 2 3 4 5 6	I II III IV V VI	VII VIII IX X XI XII
Group B	7 8 9 10 11 12	VII VIII IX X XI XII	I II III IV V VI

However, in actual presentation every figure was repeated a number of times in order to give the appearance of a memory experiment and to provide more perfect learning, and the manner of repetition was so arranged that it gave the experimenter some definite order of sequence but provided the observer with no cue as to what to expect in the next exposure. In general, the entire arrangement runs thus: the simple-series is repeated three times; their nonsense syllables are given three times to test the observers' memory of the figures; then follow three repetitions of the first half of the control-series mixed with the simple-figures; then the test-figures are mixed with the last half of the control-series and with the nonsense syllables for the previously given control-figures; and so forth. It might not be too cumbersome to reproduce the entire sequence of figures as they were presented to the observers of Group A. That for the observers of Group B can be easily surmised by making appropriate substitutions of the numerals 1, 2, 3, etc. by 7, 8, 9, etc.

2 5 3 1 6 4 VII 3 5 2 4 6 1; 2 5 3 1 6 4 nonsense syllables for 3 5 2 4 6 1, 2 5 3 1 6 4 3 5 2 4 6 1 VII; 6 4 VIII 3 1 IX 2 5 VII ns3 VIII ns6 IX ns1 VII ns5 VIII ns2 IX ns4; I nsVII X II nsVIII XI III nsIX XII IV nsVII X V nsVIII XI VI nsIX XII; I nsVII II nsVIII III nsIX X I XI II XII III IV nsI V nsII VI nsIII IV nsI V nsII VI nsIII X nsIV XI nsV XII nsVI. (ns stands for nonsense syllable.)

The observer was seated comfortably at a small table, on which were placed a pile of stimulus cards, a pad of white paper, a black and a yellow pencil. Instructions were then given orally as follows:

"This is a simple memory experiment. You have in front of you a set of cards each with a figure of some kind in the middle and a nonsense syallable at the bottom. All the figures are dotted but they vary in size and complexity. At the signal 'begin,' the top blank card will be removed, and you are to look at the figure thus uncovered and study it until it is covered up again. Try at once to reproduce, in either dots or continuous lines, what you have seen on the pad provided for the purpose. Indicate by the additional use of the yellow pencil the individual units or sections into which you split up the figure. Cover up the result of your attempt as soon as you finish and write an introspection of it; that is, a descriptive account of the figure as it appeared to

you and of the mental processes involved. When this is done, you are ready to have another trial of the same figure, and you reproduce and introspect as before; so on till you have the figure complete and correct. At a later time you will relearn the same figure. You are allowed as many trials as you need at each learning or relearning of a figure. Then a new figure follows. Proceed with it as you did the previous one. In connection with each, try to memorize the name in the form of the nonsense syllable. After several repetitions or presentations of a figure, you will be tested as to how much of it you can reproduce from memory when you are shown a nonsense syllable alone. In that case, copy the nonsense syllable and supply the figure belonging to it."

The very first stimulus-figure was a sample one constructed along the same principles as the more complex figures but containing no simple-figures. At the completion of this one, questions were asked and answered between the experimenter and the observer concerning the exact procedure, to make sure that every point was clearly understood by the latter. Then the experiment began. The simple figures were each exposed for 5 seconds. For the complex figures, 15 seconds were given.

In addition to instructions concerning the procedure, the observers were requested to refrain from discussing the nature and content of the experiment with other observers or with friends. So far as schedules allowed, two or three, and occasionally four, observers worked in the room at the same time. The individual tables were so placed that stimulus cards were within easy reach of the experimenter.

It took each observer approximately ten hours to complete the experiment. Those were distributed in five two-hour periods within one week, as far as his schedule permitted. The amount of work covered by each experimental period is shown for Group A above where each period's work is bounded by semi-colons.

Altogether 138 observers took part in these experiments, 29 of whom were students secured from the University bureau in charge of the F.E.R.A. project and the rest were members of the class in Introductory Physiological and Experimental Psychology. All of them were naïve observers, so far as the purpose of this experiment was concerned.

Results

Learning of the Simple-series as Establishment of Experience.—Before making a survey of the main results of the experiment with respect to the rôle of past experience, it would be instructive to obtain an account of how the figures of the simple-series itself were described and learned, for the sake of a better understanding and analysis of the results concerning the manner in which the figures of the masked-series were apprehended. To study the rôle of past experience in perceiving masked figures, it is imperative that we know what the past experience consisted of and how it was established.

As it has been explained in the section on Material and Procedure, the simple-figures, the apparently more complex figures and their nonsense-syllable names appeared to the

observers to be all mixed in order. For them the task was alike throughout, that of reproducing and of describing, whether the stimulus figure be simple or complex, present or replaced by a nonsense syllable.

Except in a relatively few cases (approximately 6 percent) where a simple-figure was reproduced correctly at the first trial, all required more than one trial for a first complete reproduction. The number of trials ranged from 1 to as many as 14. The following table will reveal both the range of the number of trials and the average for each simple figure.

As should be expected, the second presentation of a figure required fewer trials for correct reproduction than the first. Only in less than 3 percent of the cases did the number of trials exceed that in the first presentation of the figure, and in more than half of the cases one trial sufficed. When a figure

TABLE I

RANGE AND AVERAGE OF THE NUMBER OF TRIALS TAKEN BY THE OBSERVERS TO LEARN THE SIMPLE-FIGURES

Figures	MYD	BOF	WEZ	YOP	QEW	DAQ	JYC	GUQ	SIJ	KEZ	HYV	GAH
Range..	1–9	1–7	1–6	1–9	1–4	1–13	2–14	2–8	2–8	1–6	2–9	1–14
Average	3.75	3.17	?.5	3.45	1.84	3.62	4.63	4.55	3.84	2.6	4.5	3.77

appeared a third time, one trial was the rule, although 1½ percent of the cases required more than one trial.

A study of how each of these twelve figures was learned showed a predominance of either a one-unit or two-unit organization depending upon the individual figure. Figure 1 was unique in that it was learned in the greatest variety of ways, so varied that it defied a clear-cut classification. It suffices to say that half of the observers learning it looked at it as consisting of two parts; less than one-third of them, as of three parts. Figure 2 was seen mostly as made up of two units, with either unit equally dominating in frequency. One-third of the observers saw it as one whole. With Figs. 3, 4, 5, and 6, the majority of the observers in each case learned in two units; the rest of them, as either one entire figure or more than two parts.

The tendency in learning Figs. 8, 9, 10, 11, and 12 was to trace the outline contour as a whole before supplying what existed inside it. Although there was found a minority of cases of observers learning the figures as two separate units or more, yet the number was not at all comparable with that of the general trend. Figure 7, like Fig. 1, was a case by itself, there being no such thing as dominating tendency. However, it was learned mostly by getting hold of a larger unit and completing the figure by adding subsequently the parts attached to it.

The outstanding feature of the introspections concerning the simple-figures is the variety of descriptions. These may be divided roughly into four classes. Some observers did not attempt to describe at all; when asked for introspections, they replied that they had nothing to say, for the figures were merely a mess of lines and curves. Another group would write about them of their own initiative, some of their expressions being as follows: "It looks like nothing on earth," "It doesn't mean a thing to me," "It has no particular significance to me," "I don't recall ever having seen anything like it before," "It looks crazy," "It meant nothing definite to me," and the like. A third group would simply tell something of their method of attack and order of procedure. These three may be considered as one class when compared to a fourth group in so far as the latter described the figures in terms of their resemblance to familiar objects, alphabetical letters and geometrical designs.

These descriptions with reference to familiar objects and designs show a wide range of variation. In what follows only those are selected in which two or more observers agree in that particular type of description, the others being discarded. Each simple-figure is represented by its nonsense syllable name, followed by the list of descriptions arranged in the order from greatest to least frequency of occurrence.

MYD—triangle, bird, leaf, flower petal, pyramid
BOF —heart, shield, leg and foot, tulip, M, V, bag
WEZ —diamond, pipe, shoe, lamp, triangle, face and hat, shield

YOP —semi-circle, rectangle, heart, V, triangles, Y, top, bud
QEW —moon, hat, crescent, banana, cup and saucer, slice, boat, semi-circle
DAQ —basket with contents, flower pot, bowl, chemical apparatus
JYC —chick or duck's head or beak, heart, triangle, Y, B, flower, egg cracked
GUQ —shield, bat, wooden shoe, star, crown
SIJ —helmet, football, mushroom, egg, image or form on base
KEZ —star, triangle, arrowhead, cat's ears, M
HYV —B, D, other alphabetical letters
GAH —waves, boat on water, wooden shoe

It can be readily seen that these comparisons pertain sometimes to one small portion of the figure and sometimes to more than one part, although in the majority of cases the descriptions are ascribed to the whole figure under consideration. In no case did an observer indicate exact similarity. The most common expressions used in this connection are: "It reminded me of," "looked something like," "seemed to suggest," "appeared like," "got impression of," "got general idea of," and so forth.

However, putting the introspections of all the observers together, one finds that these simple-figures were quite unfamiliar to them and did not obtain one common response from them.

The Influence of Past Experience upon Subsequent Experience.—Sixty-one observers were employed in the first part of the experiment, thirty in Group A and thirty-one in Group B. Group A was given Figs. 1–6 as simple-figures and consequently Figs. I–VI as test-figures and Figs. VII–XII as control-figures. Group B had Figs. 7–12, VII–XII and I–VI for the respective purposes. Results for Groups A and B are shown in the first part of Table II.[5] In this Table, 'T' stands for test, and 'C' for control. T1 under ZIF means that the Fig. ZIF is the first test figure in the series, T2 the

[5] Results for individual observers not reproduced here may be obtained from the University of Michigan Library at request.

TABLE II

'SUCCESSES' AND 'FAILURES' MADE BY DIFFERENT GROUPS

Group		ZIF	XUR	QOW	ZUB	GID	KOJ	QAP	CEF	GIW	POQ	LAJ	XEH
A	Class	T1	T2	T3	T4	T5	T6	C1	C2	C3	C4	C5	C6
	Successes	6	14	23	12	18	28	0	0	0	5	0	0
	Failures	24	16	7	18	12	2	30	30	30	25	30	30
B	Class	C1	C2	C3	C4	C5	C6	T1	T2	T3	T4	T5	T6
	Successes	0	0	3	0	0	3	9	9	5	21	5	22
	Failures	31	31	28	31	31	28	22	22	26	10	26	9
A_1	Class	T5	T2	T4	T1	T3	T6	C5	C2	C3	C4	C1	C6
	Successes	2	6	15	1	7	13	0	0	0	1	1	0
	Failures	15	11	2	16	10	4	17	17	17	16	16	17
B_1	Class	C5	C2	C4	C1	C3	C6	T5	T2	T3	T4	T1	T6
	Successes	1	0	1	0	0	0	6	2	3	15	1	14
	Failures	18	19	18	19	19	19	13	17	16	4	18	5
C	Class	T1	C4	T6	C3	T4	C6	T3	C2	T2	C5	C1	T5
	Successes	5	0	18	0	14	2	9	0	6	2	0	17
	Failures	15	20	2	20	6	18	11	20	14	18	20	3
D	Class	C1	T4	C6	T3	C4	T6	C3	T2	C2	T5	T1	C5
	Successes	0	13	1	10	0	21	0	9	0	19	3	1
	Failures	21	8	20	11	21	0	21	12	21	2	18	20
$A+A_1$	Class	T	T	T	T	T	T	C	C	C	C	C	C
	Successes	8	20	38	13	25	41	0	0	0	6	1	0
	Failures	39	27	9	34	22	6	47	47	47	41	46	47
$B+B_1$	Class	C	C	C	C	C	C	T	T	T	T	T	T
	Successes	1	0	4	0	0	3	15	11	8	36	6	36
	Failures	49	50	46	50	50	47	35	39	42	14	44	14

second test figure in the series, and so on. The number of 'successes' under each item represents the number of cases in which the masked simple-figure is apprehended as a unit. The number of 'failures' represents the number of cases in which such apprehension fails to take place.

A glance at the results of the first two groups in the Table would reveal the enormous descrepancy between the results from the test-figures and those from the control-figures. The former were often apprehended in such a way that the simple-figure learned before now forms a unit by itself distinct from the other parts of the total complex pattern, whereas the latter figures were seldom apprehended in such a manner. Thus, ZIF was divided into sections with YOP as a unit by six observers in Group A who received it as a test-figure, but by none in Group B who had it as a control-figure. In the

same way the small unit HYV included in QAP was grouped as a unit by nine observers in Group B who had had HYV in their previous experience but by none in Group A. The few cases where the control-figures were divided with the included simple-figures as individual units occurred with POQ by Group A and QOW and KOJ by Group B. If past experience did not operate, either the amount of positive results in the test-figures ought to be small and approach that in the control-figures, or the number of control-figures apprehended in similar ways as the test-figures ought to increase to a comparable amount. Neither of these was what actually took place.

Similar procedure was used with another group of observers. Changes were introduced in the arrangement of the order of presenting the test-figures. The positions of ZIF and ZUB were exchanged for Group A and those of QAP and LAJ were exchanged for Group B. These observers may be called Group A_1 and Group B_1 respectively. The purpose for the shift was to find if a simple-figure contained in a test-figure occurring at the beginning of the series and detected with difficulty would be more easily detected when placed in the middle of the series and immediately after one whose masked-figure was easily found out; and to see if a test-figure occurring in the middle of the series would become more 'difficult' when placed at the beginning of the series. Thirty-six observers took part in this, seventeen in A_1 and nineteen in B_1. The results are shown in Table II following those for Groups A and B.

Here again the results for each figure are different depending upon whether it is a test-figure or a control-figure. If the small figure contained in it had been learned as a simple-figure, the larger figure is likely to be organized in such a fashion that the small figure forms an independent part. With the control-figures, however, only a small percentage of that kind of organization is recorded. The results on the exchange of positions of test-figures are going to be reported in a later section.

Forty-one observers were employed in a third part of the

experiment, in which the same figures were used but the distribution and arrangement of the figures were changed. On the basis of the results of the previous parts of the experiment, the twelve complex figures were arranged in order from the greatest to the least difficulty, so far as the degree of ease of solving the test-figures in terms of the simple figures was concerned, and then divided into two equal sets on the same basis. Therefore two changes were introduced: (1) the two divisions of six figures each were more comparable in their 'degree of coherence' and (2) the arrangement of each section was strictly in the order of decreasing 'difficulty.' They may be called C and D series. The C series of figures consisted of ZIF, GIW, QAP, GID, XEH, and QOW; the D series, LAJ, CEF, ZUB, XUR, POQ, and KOJ. Consequently, the simple-figures for learning for the C group of observers were YOP, JYC, HYV, BOF, SIJ, and WEZ; for the D group of observers, GAH, GUQ, DAQ, MYD, KEZ, and QEW.

Twenty observers of the C group had the C figures as test-figures and the D figures as control-figures; twenty-one observers, the D figures as test-figures and the C figures as control-figures. The results are given in the lower middle section of Table II.

Again there is a great difference in the results of the experimental and the control divisions. Only 6 cases out of the total 246 of the controls showed a grouping as units like the simple-figures of which they had had no experience, whereas 144 cases out of the experimentals segregated the figures learned before.

At the bottom of Table II are found the results for Groups $A + A_1$ and $B + B_1$. The results for all the three experiments are summed up in Table III.

Compared to the number of 'successes' of the division with experience, the amount of 'success' of the non-experience division is negligible. For the former, the amount of success is 48.5 percent; for the latter, 2.5 percent. These comparisons lead one to the position that past experience of certain forms is not without influence in the apprehension of other visual patterns.

TABLE III

TOTAL NUMBER OF 'SUCCESSES' MADE BY ALL THE OBSERVERS

	ZIF	XUR	QOW	ZUB	GID	KOJ	QAP	CEF	GIW	POQ	LAJ	XEH
Test	13	33	56	23	39	62	24	20	14	55	9	53
No. of Os...	67	68	67	68	67	68	70	71	70	71	71	70
Percent of Os.....	19	49	84	34	58	91	34	28	20	77	13	76
Control....	1	0	5	0	0	5	0	0	0	8	1	1
No. of Os...	71	70	71	70	71	70	68	67	68	67	67	68

The series of complex figures were learned in various ways according to the construction of each individual figure. Figs. I, X, and XII were learned largely by starting with the top or left portion, going to the lower right, and finishing by correcting the connections in the middle. Figs. II and IX, however, were usually learned either by proceeding from the bottom around the left to the right or by making an outline contour and filling in the center section. On Figs. III, IV and V, the observers began with the top working to the bottom. Figs. VI and VII were learned in the left-right direction. There was not any one dominant tendency in Figs. VIII and XI unless it was from the boundary to the internal parts.

In all these figures, the strong tendency next to the dominant one dealt with in the last paragraph was to proceed from the general outline of the figures to the details within. A second thing to note is that in those figures whose hidden simple-figures were detected at the outset, the method of learning was to use that as a basis and to build the rest around it.

One third of the observers showed a tendency to persist in using the same method in most of the complex figures: 26 of them inclined to learn most of the figures by getting the outline contour and then completing the internal parts; 7 by starting with the left-hand portion; 6 by proceeding down from the top; and 6 by crowding everything into the picture from the beginning.

In connection with the discovery of simple-figures, introspections were recorded by the observers to the effect that a former or familiar figure had been recognized. Some of the more common expressions were: "Figure so and so is in the center," "I noticed the pattern drawn before." "I noted a similarity between the encircled unit to one of the simpler figures," "It contains a simple design learned before," and so forth. Not infrequently were the nonsense syllable names of the simple figures given, mostly correctly, for if the name did not appear immediately, the observer could always resort to such vague expressions as previous figures or former designs. Such introspections were, of course, absent from the observers working on the control figures.

As to how many of the 'successful' cases were accompanied by the statement in the introspections that a simple-figure was recognized, we have Table IV giving the number of ob-

TABLE IV

NUMBER OF OBSERVERS REPORTING RECOGNITION OF SIMPLE-FIGURES

Groups According to the Number of Successes	Number of Observers Reporting Recognition of Simple-figure in						
	0 Test Fig.	1 Test Fig.	2 Test Fig.	3 Test Fig.	4 Test Fig.	5 Test Fig.	6 Test Fig.
Os. making 6 succ.					1		6
Os. making 5 succ.		3	2	2	2	11	
Os. making 4 succ.	3	4	1	6	9		
Os. making 3 succ.	10	1	4	12			
Os. making 2 succ.	8	8	19				
Os. making 1 succ.	8	8					
Os. making 0 succ.	10						
Total	39	24	26	20	12	11	6

servers in each of the seven groups making mention of the fact in the given numbers of cases. The Table reads thus: one observer in the group making 6 'successes' mentions having found a simple-figure learned before in 4 out of 6 of the test-figures, 6 observers so state in all 6 of the test-figures; 3 observers in the group making 5 'successes' mentioned the fact in one case each, 2 observers each in 2, 3 and 4 cases, and 11 of them in 5 test-figures; and so forth.

Altogether 275 'successes' in a total of 401 were mentioned in the introspections, making 68 percent.

Variability in the Effectiveness of Past Experience.— Throughout the different parts of Table II, we notice a difference in the degree of 'success' among the various test-figures, as measured by the relative amount which persists in the various experiments. In Groups A and B the different numerical figures as the sum of positive cases for the test-figures reveal a difference in the ease with which the simple-figures were recognized. The increasing order of ease in Group A runs from ZIF with its six positive cases, through ZUB, XUR, GID, QOW, to KOJ, which has twenty-eight positive cases out of thirty. The test-figures for Group B are ranked from LAJ and GIW with five successes each, through QAP, CEF, POQ, to XEH with its twenty-two successes. This variation in the rôle of past experience exists doubtlessly as a result of the relative ease of dissolving the different test-figures into the simple-figures and the parts making up the camouflage.

In Groups A_1 and B_1, the likelihood of a masked figure being grapsed is again shown to be dependent upon the degree of coherence of that test-figure. That may be graded by the number of 'successes' for each. The order for Group A_1 is ZUB, ZIF, XUR, GID, KOJ and QOW; that for Group B_1, LAJ, CEF, GIW, QAP, XEH, and POQ. Here in Group A_1 as before in Group A, ZUB and ZIF give the least results. However, the result in this particular group is proportionally much less all through the different figures, except QOW, than that in Group A. Turning to Group B_1, we find as compared to Group B also a general proportional reduction of success throughout, except in POQ and XEH where an increase is shown. This may be conjectured to be due to an inferiority on the part of these observers or to chance with the smaller size of the groups.

This increasing order of success in the test-figures exists also with Groups C and D in agreement with the results of the previous experiments. In fact, the order as achieved by Groups C and D is practically the same as that by Groups

$A + A_1$ and $B + B_1$. On the whole, LAJ, ZIF and GIW stand first in difficulty; CEF, ZUB and QAP come next; XUR and GID are medium; and XEH, QOW, POQ and KOJ seem the easiest. These all tend to show that the degree of coherence is not without its potency in determining the effectiveness of past experience.

Effectiveness of Past Experience and Attitude of Search.—A question might be raised concerning the factor of a search-attitude contributing to the successful detection of the masked figures in others succeeding any figure thus analyzed. It is asked, for instance, whether after the first successful finding of the masked figure, the observer might not become enough aware of the general situation to have his naive attitude completely changed into one of hunting for the simple-figures. The answer is that they very well might adopt such an attitude of seeking out the small figure. Although we cannot get down to the actual amount involved, on account of danger of giving suggestion by questioning, we have some evidence that search does exert its influence upon the finding of the simple-figures.

Evidence may be found in the introspections. On the test-figures, only three observers, namely, 37, 39 and 125, recorded an effort at search. Since the last three control-figures do not occur until one or more test-figures have been presented, any act of looking for the masked-figures may be shown in these last control-figures and recorded in the introspections. Thus we find ten observers say that they tried to see familiar units in those figures but could not recognize any of the simpler figures.

However, this does not alter our general position bearing on the influence of past experience. Considering only the first 'success' for each observer and leaving out all the succeeding ones, we still find that there are in the experimental groups a disproportionately greater number of 'successes' than in the control groups. Table V shows such results. The few 'successes' of the control figures are given in parentheses.

Thus, eliminating any results from circumstances which

TABLE V

NUMBER OF OBSERVERS MAKING 'SUCCESSES' WHICH ARE NOT PRECEDED BY OTHER 'SUCCESSES'

Groups	ZIF	XUR	QOW	ZUB	GID	KOJ	QAP	CEF	GIW	POQ	LAJ	XEH
A	6	9	11	0	2	2	9	6	1	(5)	0	3
B			(3)			(3)				7		
A_1	0	6	5	1	3	2	0	2	3	(1)	(1)	0
B_1	(1)		(1)							9	1	
C	5	3	1	2	3	(2)	3	7	3	(2)	3	4
D			(1)			0				6		(1)
Total Test	11	18	17	3	8	4	12	15	7	22	4	7

might present a mixture of the influence of previous experience and of a search-attitude and considering only the one first success by each observer, we yet obtain a result showing by a wide margin the effect of past experience.

The influence of search can possibly be tested also in the 'success' of the observers toward the second presentation (or relearning) of test-figures whose small figure had been overlooked at the first presentation (or first learning). For, aside from ten observers who were absolute 'failures,' all had one or more 'successes' in the first presentation of the test-series. That may lead them, when a figure on which they had failed comes around again, to assume a different attitude and to be more likely to seek out the hidden simple-figure.

The facts are as follows. Of 10 observers who had no 'success' on first presentation, one gave 1 success later. Of 16 observers making 1 success each on first presentation, 3 added a total of 4 on later presentation. Of 35 making 2 each on first, 13 added a total of 26 later. Of 27 making 3 each on first, 13 added a total of 21 later. Of 23 making 4 each on first, 11 added a total of 17 later. Of 20 making 5 each on first, 3 added 1 each later. For only a small part of the observers were the additions sufficiently large to indicate positive effects of an attitude of search. Most of the additions might have occurred as a result of study in relearning.

It may be noted that, if we add the 72 'successes' on later

presentations to the 401 which occurred on the first presentation, we have a total of 57 percent successes on test-figures.

In changing the order of presenting the test-figures for the observers in Groups A_1 and B_1 from that in A and B, it was expected to find a variation in the results of the figures involved in the change. To be sure, a figure like ZUB occupying a middle position in the series A underwent a decrease in its amount of success when moved to the head of the series in A_1, but the figure ZIF exchanging position with it also underwent a reduction in success. Any clear analysis here is obscured by the fact that this group of observers happened to be of a poorer caliber, as shown in the general low level of success through all the figures. A comparison of items in B and B_1 shows that the exchange of position between LAJ and QAP has brought about a marked decrease in the success of LAJ when it assumes a first position preceded by none but the control figures, and a proportionally slight increase in the success of QAP which now occurs in the middle of the series following POQ, one of the very easy figures.

Not a few observers remarked that the presence of the known figure made easier the mastery of the larger figure, serving as a basis upon which the rest was built, although the fact is sometimes mentioned that the familiar figure made it hard for the rest to be placed in correct relation with it.

As to the general attitude, favorable or otherwise, of the individual observers toward the task, observations were made and recorded by the experimenter. These, combined with the introspections, gave three classes of attitude, namely, interest, abhorrence and indifference. The majority of them have the indifference attitude, showing no particular enthusiasm for, nor any hostility toward, the type of work. Next in number comes the group who demonstrate a decided interest in it. Then there are those who abhor it because, they aver, it is uninteresting or too difficult.

From this classification is discovered an interesting fact that success in finding the simple-figures in the camouflage seems to bear a relation to the amount of interest displayed. Observers who worked with enthusiasm made, on the average,

the largest, and those with dislike, the least number of successes. Those who were indifferent stood in between.

The Effect of Past Experience Upon the Speed of Learning.— The complex patterns under which the simple-figures are masked differ in the ease with which they are learned. The number of trials for a first successful reproduction of the figures range from 1 to 22. The average number of trials for each figure, separate as test- and control-figure, is shown in Table VI, which reveals several important facts. First,

TABLE VI

AVERAGE NUMBER OF TRIALS FOR LEARNING THE COMPLEX FIGURES

	ZIF	XUR	QOW	ZUB	GID	KOJ
Test						
Positive cases	3.69	5.03	3.59	5.21	4.38	3.18
No. of Os	13	33	56	23	39	62
Negative cases	4.57	5.77	5.91	6.45	5.04	4.00
No. of Os	54	35	11	45	28	6
Control	5.28	6.96	5.70	6.99	5.28	4.41
No. of Os	71	70	71	70	71	70
	QAP	CEF	GIW	POQ	LAJ	XEH
Test						
Positive cases	5.41	5.95	7.42	6.27	4.67	5.92
No. of Os	24	20	14	55	9	53
Negative cases	8.10	7.52	7.48	6.75	5.33	6.11
No. of Os	46	51	56	16	62	17
Control	8.76	8.18	7.89	7.37	6.01	6.66
No. of Os	68	67	68	67	67	68

any figure serving as test-figure requires fewer number of trials to master than as a control-figure. That is, an observer who has had past experience of a part of a complex figure takes fewer trials to complete the reproduction of that figure than another observer who has had no experience of any of its parts.

Secondly, a test-figure whose masked-figure is detected requires fewer number of trials to learn than one in which an observer fails to notice the masked-figure.

Thirdly, it is interesting to note that in eleven out of the twelve instances the average number of trials required to

learn the control-figure is larger than that to learn a test-figure whose included small figure has failed to be found. The two situations are similar in the respect that no past experience has been directly utilized, one having no past experience to make use of and the other failing to call upon it in a novel circumstance. It would seem probable that the difference is due to practice effect.

Practice effect is manifest in the learning of these nonsense figures. Its presence is shown by comparing the average of the number of trials a figure requires when it occurs early in the series of twelve with the average when it comes later in the series. This comparison is made possible by the shifts in the positions of the individual figures in the different experiments. Such averages are shown for control-figures in Table VII.

TABLE VII

AVERAGE NUMBER OF TRIALS FOR CONTROL-FIGURES IN DIFFERENT POSITIONS

Positions	ZIF	XUR	QOW	ZUB	GID	KOJ	QAP	CEF	GIW	POQ	LAJ	XEH
1 Av.....	5.62			7.26			9.00				6.89	
Os.....	52			19			30				37	
2 Av.....		7.12						8.18	9.81			
Os.....		50						67	21			
3 Av.....			5.80	8.10	6.05		9.05		7.04			
Os.....			31	20	19		21		47			
4 Av.....		6.55	5.68	6.13	5.24					7.04		
Os.....		20	19	31	21					47		
5 Av.....	4.37				4.84		8.00			8.15	4.93	7.70
Os.....	19				31		17			20	30	21
6 Av.....			5.57			4.41						6.21
Os.....			21			70						47

If we compare the trials required when a given figure is used as an early control with trials required when the same figure is used as a later control, we find in general a less number of trials with the later position. Now according to the general procedure, it will be remembered, the first three of the twelve complex figures are control-figures, then test- and control-figures alternately, and lastly three test-figures. Any

figure therefore occurs later in the whole series when used as test-figure than when used as control-figure of the same number. Other things equal, the effect of practice would thus be to give more rapid learning for test-figures.

Lastly, the relation between the complexity of the figures as measured by the number of trials required to learn when used as controls and the difficulty of finding their included simple-figures is found to be slight, the coefficient of correlation between the data of Tables VI and III, obtained by the rank-difference method, being .39 with a P.E. of .17, which shows extreme unreliability.

The Readiness with Which the Simple-figures are Isolated.— It is time at this point to bring up for consideration the factor of readiness in segregation, in the sense of isolating the simple-figures at the first trial or during subsequent trials. Due to the fact that the task set for the observers was that of trying to commit the visual structure to memory, they were most intent at picking out any unit or units which could be readily grasped. Many of the observers followed a definite order of atack; *i. e.*, some made it a rule to start with the entire contour, others with the top or the left side, with the result that the accuracy of reproduction was emphasized at the expense of readiness in appropriate segregating. In order to discover how the different figures compare in this respect, the average number of trial at which the masked figure is unmasked is found and tabulated in Table VIII.

Aside from the averages, the Table gives also the number of 'successes' at first trial (Item 2), the percentage of observers making first-trial successes (Item 3), and success at first trial in percentage of total successes (Item 4).

The number of 'successes' in the first trial of the control-figures is only 3, so small a number that it has been omitted from the Table. As to that for the test-figures, it is shown in the second item of the Table, and the total of such successes is 231.

As should be expected, there is a direct relationship between the first and last items: the fewer the average number of trial at which the part-figure is isolated, the larger the

TABLE VIII

COMPARATIVE READINESS WITH WHICH MASKED FIGURES ARE APPREHENDED

		ZIF	XUR	QOW	ZUB	GID	KOJ
1. Av. no. of trial at which the masked fig. is isolated	Test....	1.92	2.12	1.53	2.43	1.44	1.16
	Contr...	4	—	4	—	—	1.6
2. No. of succ. at 1st trial	Test....	6	13	39	8	28	55
Total no. of Os.....		67	68	67	68	67	68
3. Percentage of Os.....		9	19	58	12	42	81
4. Success at 1st trial in percentage of total successes	Test....	46.1	39.4	69.6	34.8	71.8	88.7
	Contr...	—	—	—	—	—	40.0
		QAP	CEF	GIW	POQ	LAJ	XEH
1. Av. no. of trial at which the masked fig. is isolated	Test....	2.33	3.45	3.43	2.13	2.33	2.32
	Contr...	—	—	—	4.62	6	3
2. No. of succ. at 1st trial	Test....	10	4	3	33	5	27
Total no. of Os.....		70	71	70	71	71	70
3. Percentage of Os.....		14	6	4	46	7	39
4. Success at 1st trial in percentage of total successes	Test....	41.7	19.0	21.4	60.0	55.5	50.9
	Contr...	—	—	—	12.5	—	—

percentage of cases of successful isolation at the first trial. Little can be accomplished by comparing the figures when they are used as controls, since there are so few successful cases; however, one point is worthy of note. In every control-figure where a 'success' is recorded, it occurs at a later trial than that in the test-figures.

If we compare the order based upon 'success' at first trial in percentage of total successes (Item 4) and the average number of trial at which the masked-figure is isolated (Item 1) in this Table with that in Table III, we find a correlation of .65 with a P.E. of .12. Thus there is a positive relationship between the readiness with which masked figures are apprehended and the percentage of successes ultimately achieved. Comparing this Table with Table VI, we see a closer relation-

ship, the coefficient being .77 ± .08 between the order of readiness in percent of first-trial successes (Item 4) and the order of complexity as measured by trials to learn the control-figures, and .84 ± .06 between the latter and the order of readiness in terms of average number of trial at which the part-figure is isolated.

The correlation between the percentage of observers seeing the masked-figure in the first trial (Item 3) and that in any trial (Table III) is found to be .94 ± .02. Between the former and the complexity of the figures as measured by the number of trials required to learn when used as controls (Table VI), the correlation is .53 ± .14.

The Influence of Shift and of Partial Success.—It may be questioned whether the 'successes' as we have obtained them are not the consequence of a constant shifting of organization on the part of the observers in their attempt to master the figures. And may they not also possibly be results arrived at by way of partial successes?

To answer the first question, we proceeded to look for 'shifts' in both test- and control-figures and then to find their relationship to the number of 'successes.' By 'shift of organization' we mean the utilization by the observer of one type of organization in one trial and an entirely different type of organization in the following trial or trials. It is not considered a shift if the observer in the first trial succeeds in reproducing half of the test- or control-figure describing it as consisting of three units, for instance, and then in the second trial completes the figure by adding on the remaining parts but retains the organization of the units secured in the preceding trial. It is definitely a shift, however, if in the second trial those units are not reproduced at all or one or two of them combine with some newer parts of the figure to become a larger whole. There are numerous ways where shifts are observed, but in all cases a shift is recorded when the organization in one trial gives way to another in a succeeding trial.

An attempt is therefore made to find the relationship between the number of shifts in the figures as controls and the number of 'successes' in these as test-figures. To avoid the

complications arising from the possibility of a smaller likelihood of shift due to success in finding the simple-figure, shifts in test-figures are not included in the consideration. If shifting is beneficial to success, then a figure conducive to shifting by controls ought to be more likely to be 'successful.' The respective numbers in question are given in Table IX.

TABLE IX

RELATIONSHIP BETWEEN THE NUMBER OF SHIFTS AND THE NUMBER OF 'SUCCESSES'

Figures	As Control-figures No. of 'Shifts'	As Test-figures No. of 'Successes'
ZIF	29	19
XUR	30	49
QOW	37	84
ZUB	26	34
GID	34	58
KOJ	25	91
QAP	49	34
CEF	43	28
GIW	37	20
POQ	32	77
LAJ	26	13
XEH	32	76

A computation, by the rank-difference method, of the degree of correlation between the number of 'shifts' and the number of 'successes' reveals complete lack of correlation, the result being $-.03$. Therefore success in discovering the simple-figures is not dependent upon the amount of shifting.

Whether shifting has anything to do with the amount of 'success' can be studied yet from another angle. Perhaps a relationship exists when the methods and achievements of individual observers are taken into consideration. If the observers making zero shift in the control-figures achieve less success in the test-figures as compared to those making an all-shift record, there might be some indication of a positive relation between the two functions with which we have been dealing. Accordingly, the observers are arranged into seven groups on the basis of the number of shifts from 0 up to 6 in the 6 control-figures, and the number of their respective successes in the test-figures is noted. These are shown in Table X.

TABLE X

AVERAGE NUMBER OF 'SUCCESSES' MADE BY OBSERVERS GROUPED ACCORDING TO THE NUMBER OF SHIFTS

Number of Shifts in Control-figures	Number of Observers	Aver. No. of Successes in Test-figures
0	17	2.88
1	24	3.08
2	24	2.60
3	19	3.10
4	21	3.00
5	11	2.45
6	22	2.82

There again appears to be no relation between the amount of shifting and the degree of success. Thus, a study of the relationship from the point of view of both the individual figures and the individual observers gives us negative results; 'shifting' cannot be held responsible for 'successes.'

After an observer made a 'successful' organization of a test-figure, isolating the simple-figure by means of the yellow mark, he usually retained its isolation during the following trials till the complex-figure was learned. There were only five cases out of the 401 'successes' of the test-figures in which the observer lost the simple-figure organization after he had once found it, besides two other cases of temporary shift and recovery of such organization. Of all the seven, however, only one mentioned in the introspections the similarity of this to the simple-figure learned before. It is likely that they did not come to a realization of the identity.

Can 'successes' be explained by the possibility of a tendency for the observers to catch on to a simple-figure from noticing a part of it in the process of learning to reproduce the complex-figure? That is, will seeing a partial simple-figure lead the observer to the getting of the whole of it? As described in a previous section, the simple-figures are likely to be learned as two parts, if not as one whole. It may be that getting one part of it in the test-figure will make the observer aware of the whole of the simple-figure and therefore lead to success.

Table XI gives the number of approximations or partials made in any trial as the first of the numerical figures in each

TABLE XI

NUMBERS OF APPROXIMATIONS OF THE MASKED-FIGURES AND NUMBERS OF 'FAILURES'

	ZIF	XUR	QOW	ZUB	GID	KOJ	Total
Test.....	33 54	19 35	4 11	18 45	16 28	1 6	91–179
Control.....	36 70	14 70	12 66	15 70	20 71	21 65	118–412
	QAP	CEF	GIW	POQ	LAJ	XEH	
Test.....	12 46	10 51	1 56	2 16	14 62	1 17	40–248
Control.....	7 68	5 67	6 68	8 59	10 66	4 67	40–395

column; the second number is the total number of cases showing no 'success,' and obtained by subtracting the number of 'successes' from the total number of observers tested on that figure.

In the test series we find a total of 131 partial 'successes' out of 828 cases in all the 12 test-figures. The total number of actual 'successes' amounts to 401 as gotten from adding the figures in Table III. Thus actual and partial successes put together come up to 532. If successes are due to jumping from a part to the whole simple-figure, then the total number of successes plus partials in the test and control situations ought to be comparable with each other, the control total consisting of mostly partials and the test total consisting of mostly wholes and few or no partials. As a matter of fact, the total for the control situation is only 179, with 21 cases of isolation of the whole simple-figure and 158 partials.

Moreover, if we omit the 'successes' and consider only the remaining cases as possible exhibits of partial successes, we have 131 partials out of 427 chances, or 30.5 percent, in the test series; and in the control series, 158 partials out of 807 chances, or 19.5 percent. Not only are the complete 'successes' mostly confined to the test series; but, relatively, there are, in the test series, more partial successes that do not lead on to complete successes. It seems impossible to explain the successes of the test-series as merely completions of the partial successes that would be there anyway.

It should be added that no partial 'success' was ever recognized and reported in the introspections as pertaining to the original simple-figures.

Since the total number of complete 'successes' in the test-figures was 401, and 231 of these were found on the first trial, there remained 170 cases of success at trials later than the first. In 37 of these 170 cases, the complete success was preceded by a partial success; in 32 of these the partial success was shown for one trial only immediately preceding the complete success. The average trial at which these partial successes occurred was 2.4. The average trial at which the 131 partial successes not followed by complete successes (131 out of a possible 427) occurred in the test series was 2.8. There is no apparent reason to suppose the complete successes arose primarily from the partial successes even in these 37 cases. The average trial at which partial successes occurred in the control series was 3.1.

Results of Second Series of Test- and Control-figures Following the First.—The twelve simple-figures were masked under different complex figures with entirely different nonsense-syllable names. These were presented without any additional instructions at the end of the main experiment to sixty-four of the observers who could afford to give the extra time. The results are not given in detail, but a few general facts should be noted.

The test cases showed 64 percent 'successes,' the control cases 6 percent. If we designate the complex-figures by the enclosed simple-figures and arrange them according to the percentage of success in the test cases, we find a correlation of .81 $\pm$ .07 between the figures in the main experiment and in this supplementary experiment. But without controls, we cannot determine clearly whether this is due to intrinsic characters of the simple-figures, or whether isolation of a given figure in one complex setting increases the probability that that same figure will be isolated in another complex setting, a direct influence of experience of the figure in a setting. That the latter is the true explanation is indicated by the fact that for any given observer the specific figures isolated in the main experiment, counting all three presentations, were usually isolated in the supplementary experiment, and in addition a few other successes occurred. It may be

observed that, counting the second and third presentation, these 64 observers showed 56 percent successes in the main experiment and added only 8 percent to give the 64 percent success in the supplementary experiment. Of the 23 cases in which a simple-figure was segregated in the controls, 9 were the specific figures isolated by the same individual observers in the main experiment and here picked out and recognized as having been experienced before; and 7 additional were all of one specific figure which was poorly embedded.

It would be of interest as a control to use the figures of the supplementary experiment with a new group of observers.

Discussion and Summary of Results

The results of this experiment point to the conclusion that past experience does make a difference in the way a complex visual pattern is organized. A great number of possibilities of dividing into recognizable units existed in the complex figures, which indeed were organized and learned in a great variety of ways. It was exactly because of this capacity for variation that a small percentage of the cases with no previous experience so organized the control-figures that the masked-figures were separated. However, the advantage of past experience enabled a great proportion of the test groups not only to recognize and utilize the learned part in the building up of the larger whole, but also to learn the complex figures with greater ease.

The habit of learning the complex figures in terms of individual lines rather than separate or overlapping units possessing definite form prevented not a few of the observers from finding the concealed simple-figure, which in the course of being committed to memory, had been conceived of as a unitary form.

When a part of the body of a simple-figure was seen deeply absorbed and imbedded in a neighboring unit, this masked-figure easily lost its identity. Often the units in question were described as geometrical patterns or objects of daily encounter, showing the strong tendency for the observers to attach meaning to the units. The supposedly nonsense

figures became meaningful in the process of learning. The manner of organization so discussed may be the coöperative result of past experience, recent and remote, and possibly native tendencies. The obstacle to a clearer understanding is that what is native cannot very well be wholly separated from what is acquired, particularly in adult observers. There are doubtless certain principles of organization that one should call native, whatever that may ultimately mean. Such are persistence of unbroken line, inclusion and exclusion (unity of contours enclosing or excluding unbroken space), belonging together of lines with any sort of homogeneity of direction, etc. Gottschaldt's figures would seem to be largely determined by such factors so that experience could have little effect.

However, our experiment is concerned chiefly with the rôle of past experience where many organizations are more or less equally possible. Whether and how native factors condition ways of organization must be further determined by experiments better suited for the purpose than the existing ones.

Our problem has been to investigate experimentally the rôle past experience of visual forms plays in the organization of more complex visual forms in which the former were included in concealment. A summary of the results is as follows.

1. The masked-figures were seen as separate units in their respective complexes twenty times more often by the experimental groups than by the control groups and seventy-seven times more often on the first trial. This great difference existed as an outcome of the former's having learned those figures in isolation. Therefore past experience plays an important rôle in our visual organization.

2. For the observers who had had past experience, some masked-figures were more easily found than others. Past experience varied in the amount of its effectiveness.

3. A search-attitude was not a decisive, but possibly a contributing factor in the discovery of masked-figures.

4. The readiness with which a masked-figure was isolated bore a relation to the total probability with which the masked-

figure was found and to the complexity of the figure containing it as measured by the number of trials required to learn.

5. Success in discovering the masked-figure during learning was not dependent upon shifting and partial success.

6. Discovery of a simple-figure in one complex setting seemed to make more probable the discovery of the same figure in another setting.

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STUDIES IN RETROACTIVE INHIBITION: VIII. THE INFLUENCE OF THE RELATIVE ORDER OF PRESENTATION OF ORIGINAL AND INTERPOLATED PAIRED ASSOCIATES

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Introduction

The learning of interpolated material operates, over an extensive range of conditions, to inhibit the recall and relearning of material which had been acquired closely prior to the interpolation. The way in which this decrement is produced is one of the most important general problems in the field of retroactive inhibition. We can hope to solve it only by a detailed examination of the conditions which determine the phenomenon. The present paper reports a study of one of these conditions.

Serial order of presentation is one factor which is uniformly common to the learning of both original and interpolated verbal series. The items of the original list are presented, that is, in the same serial positions at every repetition, and those of the interpolated list are, likewise, presented in the same order throughout. It is possible that this constant factor of common serial position may play a part as one of the conditions of interference. That it is only one among several is evident from the fact that inhibition is a function of the character of the material under conditions which maintain constancy of relative serial position. Its importance cannot, however, be assessed *a priori;* it might, conceivably, be great and it might be minimal. The present experiment constitutes one test of its influence.[1]

[1] This experiment has been assisted by a Grant-in-aid from the National Research Council to the senior author while at the University of Missouri. We are indebted to Professor Arthur W. Melton and Dr. Henry N. Peters of the University of Missouri for valuable critical suggestions.

Experiment I

Procedure.—The factor of serial position may be cancelled out by rotation of the items to be learned so that at each presentation of the list a different item occupies any given position. By this means a constant connection is prevented from being formed between item and position, and a comparison of retroactive inhibition under this condition and under the usual one of fixed serial position gives a measure of the influence of position. The rotation can, of course, be applied only to the original list, only to the interpolated list, or to both of them. Serial learning by the anticipation method, which has been used in the other experiments of this series which have studied retroaction with verbal lists,[2] does not lend itself well to a rotation of items within a list. The method of paired associates is well adapted to it and has been used.

The learning materials were pairs of nonsense syllables and two-syllable nouns, as Z E Q—COMET, in lists of 10 pairs each. They were read to the subject at the rate of one pair every 3 seconds by an experimenter who had practiced reading them until the rate and the other features of the method had become habitual. The first members, the nonsense syllables, were spelled and the second members, the words, were pronounced. In order to facilitate perception of each pair as a unit the experimenter's voice was lowered slightly at the end of each second member and there was a slightly greater interval between pairs than between the members of a pair. The three-second rate includes the total time spent per pair, however, so that the total time per list is 30 seconds. An interval of about 5 seconds elapsed between presentations. Experimenter and subject were separated from each other by an opaque cloth screen to prevent the subject from being distracted by the writing and other movements of the experimenter. A mirror was arranged to give the experimenter full view of the subject without the subject's knowledge. It would be possible for a subject, when thus concealed from the direct view of the experimeter, to write down the pairs as they were read to him and to read from his record on recall. The mirror permitted a complete check upon this possibility. Three subjects, in opposition to instructions, wrote down the pairs, and their data, both for that condition and for all others on which they had already served, were discarded.

In every condition of the experiment the lists were presented 5 times. Immediately after the fifth presentation the first members were given alone and the subject was instructed to recall the right associate. Five seconds were allowed for the recall of each one. If a recall had not been made by the end of that time, the next syllable was presented. The timing was done by means of a large seconds-timer directly in front of the experimenter. It is, of course, impossible to maintain rigid accuracy of timing under this method of presentation. Practice and care on the part of the experimenter can, however, reduce deviations to a minimum and there is no reason to believe that this remaining minimum has had any significant influence upon the results.

Two different procedures have been employed with respect to order of presentation in learning. In one case the pairs have been presented in the same serial order at each of the 5 presentations and at the test. In the other they have been given in a

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[2] J. A. McGeoch, Studies in retroactive inhibition: I. The temporal course of the inhibitory effects of interpolated learning. *J. Gener. Psychol.*, 1933, **9**, 24–42; II. Relationships between temporal point of interpolation, length of interval, and amount of retroactive inhibition. *Ibid.*, 1933, **9**, 44–56; and J. A. McGeoch and G. O. McGeoch, VI. The influence of the relative serial positions of interpolated synonyms. This Journal, 1936, **19**, 1–23.

changing order such that each pair appears in a different serial position at each presentation and at the test. At each presentation after the first the list began with a different pair and the successive pairs were in the reverse order of their appearance in the list at the first appearance. Let the order at the first presentation be from 1 to 10; at the second it was 5-4-3-2-1-10-9-8-7-6; at the third it was 7-6-5-4-3-2-1-10-9-8, and so on.

In each case the subjects were told in advance which procedure would be used (but not the plan of constructon of the changing order), and were instructed to disregard order and to attempt only to connect the second member of each pair with the first. In relearning the procedure duplicated in each case the one followed in learning. Prior to the beginning of the experiment each subject was given 2 practice periods at each of which he learned 2 lists, one with presentation in the same order throughout and the other with presentation in a changing order. The sequence of the orders was counterbalanced to give each equal average position.

The experiment comprises 2 rest and 4 work conditions. In the rest conditions the subject was given 5 presentations and a test, followed by 10 minutes of reading jokes, under instruction to mark the 3 best on each page, and then by relearning to 3 perfect recalls in succession. The procedure in relearning duplicated that in learning except for the fact that a test (presentation of first members) was given before the list was presented again and also after each presentation until the 3 perfect tests or recalls were reached. Tests and presentations thus alternated during relearning, with a test coming first. In the work conditions the 5 presentations for learning and the test were followed 30 seconds later by 5 presentations and a test of a second or interpolated list. The remainder of the ten-minute interval (slightly more than 5 and ½ minutes), corresponding to the one under rest, was filled by the reading of jokes, after which the original list was relearned to 3 perfect recalls in succession and 2 presentations and tests of the interpolated list were given. The latter were for the purpose of preventing a suspicion on the part of the subject, as the experiment progressed, that the interpolated lists were less important and deserved less effort than the original lists.

The character of the 6 conditions can most easily be seen from the following outline in which SO and CO stand for same and changing order, OL and IL for original and interpolated list, L and RL for learning and relearning:

Rest 1.....	L_{SO}	Reading 10 min.	RL_{OL}	
Work 1.....	L_{SO}	L_{SO}	Reading 5.5 min.	RL_{OL}	RL_{IL}
Work 2.....	L_{SO}	L_{CO}	Reading 5.5 min.	RL_{OL}	RL_{IL}
Rest 2.....	L_{CO}	Reading 10 min.	RL_{OL}	
Work 3.....	L_{CO}	L_{CO}	Reading 5.5 min.	RL_{OL}	RL_{IL}
Work 4.....	L_{CO}	L_{SO}	Reading 5.5 min.	RL_{OL}	RL_{IL}

The features already described, such as number of presentations, have been omitted. It can be seen that there is a rest condition for each order of presentation and that, likewise, there is an interpolation of each kind following each order. Except for order of presentation the 4 work conditions are identical and except for the interpolated lists the work and rest conditions are identical.

Thirty subjects, college students with no prior experience upon this experimental procedure, went through the 6 conditions in an order counterbalanced to give each condition equal average position. Ten different lists were employed and these were distributed over the 6 conditions in an order counterbalanced to average out differences in difficulty. Two different experimenters each took 15 subjects through all conditions. The experimenters were carefully trained in the procedure before the beginning

of the experiment and were supervised throughout. The results of the 2 groups have been worked up separately, have been found to agree closely, and have, therefore, been combined in the tables which follow.

The amounts learned during the 5 presentations of both original and interpolated lists have been measured in terms of the number recalled at the immediately following test. Retention has been measured in terms of 4 different scores: (1) Recall 1, the number of right associates at the first test after the ten-minute interval and before the pairs had been presented again; (2) Recall 2, the number of right associates given at the second test, after the first relearning presentation of the pairs; (3) Relearning 1, the number of trials required to reach one perfect test after the interval; and (4) Relearning 2, the number of trials required to reach 3 perfect tests in succession. The total numbers of right associates and of incorrect associates given on relearning tests have been found but will not be reported. The total numbers of right associates show the same tendencies as do the 4 major scores and the incorrect responses show no variations of importance.

Results.—It is, first of all, necessary to inquire whether the constant frequency of 5 presentations has yielded comparable degrees of learning as measured by the immediately following test. The data show certain differences (Table I), which

TABLE I

NUMBER OF ITEMS LEARNED UNDER EACH CONDITION DURING THE 5 PRESENTATIONS. $N = 30$

List	Condition					
	Rest 1 SO*	Work 1 SO–SO	Work 2 SO–CO	Rest 2 CO	Work 3 CO–CO	Work 4 CO–SO
Original: M	7.60	8.43	8.70	5.46	5.86	5.90
σM	.45	.36	.29	.42	.48	.39
Interpolated: M		8.13	5.90		5.44	8.20
σM		.39	.51		.42	.36

* The letters SO and CO refer to same order and changing order of presentation. Under the work conditions the first symbol signifies the order of the original list and the second that of the interpolated.

require discussion. The amounts of the original lists learned when the order of presentation is the same throughout differ somewhat from each other, but the largest difference, that of 1.10 between Rest 1 and Work 2, has a standard error of .53 and we may consider degrees of learning with this method of presentation to be sufficiently close together for our purpose. The amounts of the interpolated lists learned under the 2 work conditions which employed the same order (Work 1 and

Work 4) are closely similar to those of the original lists learned under this order.

When we turn to the amounts learned with a changing order of presentation, the original and interpolated lists are found not to differ importantly among themselves but to have consistently and for the most part reliably lower means than the lists learned with the same order throughout.[3] The subjects were told that the serial order of presentation was unimportant, that they were to pay no attention to it and were to attempt to connect the second member of each pair with the first member without regard to the position of the pairs. In spite of this instruction maintenance of the same order throughout is consistently accompanied by higher degrees of learning than is a constantly changing order.

We have, thus, 2 groups of means the magnitudes of which are functions of mode of presentation. The means within each group are sufficiently close together so that the differences may be disregarded in interpreting the data upon retention. The 2 groups, however, differ importantly in degree of learning and the difference must be considered in any interpretation of the retention data.

We shall proceed, then, to the data upon retention, which are the major concern of the experiment. The basic data (means and sigmas of the means) are presented in Table II and the major results can be seen there by making a direct comparison of the means. It is, however, desirable to have, as well, a statement of relative amounts of inhibition under the different conditions. This has been obtained by the formula *Rest Score minus Work Score/Rest Score*, using the mean recall and relearning scores of Table II.

We shall consider first the results when the original lists are presented in the same order throughout. It can be seen from the first row of percentages in Table III that the 5 presentations of an interpolated list in the same order produce inhibition in terms of each of the 4 measures of retention. The difference between the means for Recall 1 under Rest

[3] It is worth noting that the means under the changing order of presentation are relatively more variable than those under the same order.

TABLE II

MEAN RECALL AND RELEARNING SCORES

Condition	Score			
	Recall 1	Recall 2	Relearning 1	Relearning 2
Rest 1 (SO)	7.16±.50	8.53±.48	1.43±.30	1.86±.33
Work 1 (SO–SO)	3.80±.45	7.50±.48	1.90±.20	2.46±.29
Work 2 (SO–CO)	6.86±.63	8.66±.41	1.43±.31	2.06±.42
Rest 2 (CO)	4.80±.42	6.90±.47	5.43± .87	7.63±1.29
Work 3 (CO–CO)	3.66±.44	6.20±.48	6.16±1.02	7.63±1.49
Work 4 (CO–SO)	4.63±.46	6.43±.45	5.66± .96	7.23±1.05

TABLE III

PERCENTAGES OF INHIBITION COMPUTED FROM MEAN RECALL AND RELEARNING SCORES

Condition	Score			
	Recall 1	Recall 2	Relearning 1	Relearning 2
Work 1 (SO–SO)	46.9	12.1	32.9	32.2
Work 2 (SO–CO)	04.1	+01.5	00.0	10.7
Work 3 (CO–CO)	23.7	10.1	13.4	00.0
Work 4 (CO–SO)	03.5	06.8	04.2	+05.2

and Work 1 is the only one which is reliable statistically (Table II). The fact, however, that the results of the 3 other measures are consistent with the result from Recall 1, where there is a reliable difference between the rest and work conditions, suggests that the interpolation in question has had a uniformly inhibitory effect.

The interpolation of an equal number of presentations in a changing order has, on the other hand, no uniform influence. Recall 1 and Relearning 2 show small amounts of inhibition, Recall 2 shows an insignificant gain, and Relearning 1 shows no effect. When the unreliability of the small differences between the means involved is taken into account, the only justifiable conclusion is that the learning of an interpolated list presented in a changing order has no important inhibitory effect upon the retention of an original list presented in a constant order.

The results show, thus, that when the original list is presented in a constant order interpolated learning with a

similarly constant order is uniformly inhibitory and that interpolated learning with a changing order has almost no inhibitory effect. It is necessary, however, to compare degrees of interpolated learning under these 2 conditions of interpolation before the conclusion stated may be accepted. The test after presentation in the same order throughout yields a mean of 8.13 adjectives, while that after presentation in a changing order yields only 5.90. The difference of 2.23 between them has a sigma of ±.64 and is paralleled by the difference in inhibition produced by the 2 interpolations. This raises at once the question whether the difference in inhibition may not be a function of the difference in degree of learning instead of being a function of order of presentation. This is, of course, a possibility and would be, if it stood alone, a barrier to any conclusion about the influence of order. It is made less probable by the fact that a list learned in the same order, when interpolated after one learned with a changing order, produces no important amounts of inhibition, while interpolated learning with a changed order produces consistently larger amounts, although the means for the 2 interpolations are 8.20 and 5.44, respectively. The difference of 2.76 ± .55 in favor of constant order of presentation is accompanied here by a decrease in inhibition. This fact makes it unlikely that the difference under discussion above is the major determiner of the results. The 2 differences of comparable magnitude and direction but accompanied by opposite results in terms of inhibition make doubtful any major reference of these results to degree of learning and leave order as the major variable. We shall, however, report direct evidence upon this point in Experiments II and III.

We shall turn now in detail to the amounts of inhibition when the original lists are presented in a changing order. Interpolation in a changing order produces retroactive inhibition in amounts between 10 and 23 percent in terms of the first 3 scores listed in Table III but produces none in terms of Relearning 2. The differences between the means at Rest 2 and Work 3 in terms of these first 3 scores are, however, statistically unreliable and the consistency of its appearance

is the chief argument for the existence of a positive amount of inhibition under Work 3. The chances that the 3 differences involved are greater than zero vary from 70 to 96 in 100.

Interpolation with a constant order of presentation yields a small amount of inhibition in terms of the first 3 measures and a small facilitation effect in terms of Relearning 2. The differences between the means involved in the computation of these percentages of inhibition are so small and so statistically unreliable that the only justifiable conclusion is that the interpolation under Work 4 has produced no important effect upon the retention of the original list. Retention is, however, consistently greater under Work 4 than under Work 3 [4] and it may be concluded that, when the original list is learned with a changing order of presentation, an interpolation learned with a changing order produces some decrement in retention while a similar interpolation learned with a constant order produces almost no decrement.

We may next compare the results of Work 1 and Work 2 with those of Work 3 and Work 4. Work 1, in which both original and interpolated lists are presented in an unchanged order, consistently yields inhibition in amounts which are larger than those found under any other condition of the experiment and particularly than those found under Work 2 and Work 4. Work 3, in which both lists are presented in a changing order, gives less inhibition than does Work 1 but somewhat more than does either Work 2 or Work 4. The two latter agree in showing almost no decrement in retention.

The failure of Work 2 and 4 to show any important amounts of inhibition may be accepted as valid, since the data are probably not weighted heavily by the differences between original and interpolated lists with respect to degree of learning. The greater inhibition under Work 1 than under Work 3 is of more uncertain validity, inasmuch as the degrees of learning of the original and interpolated lists in Work 1 are larger than the degrees of learning of the corresponding

[4] It has already been noted that the degree of interpolated learning is greater under Work 4 than under Work 3. In spite of this fact Work 3 produces the greater inhibition.

lists in Work 3, and there is nothing in terms of which the influence of the differences may be estimated. This can be done, however, from the data of Experiments II and III.

As the results stand, retroactive inhibition is greatest when both original and interpolated lists of paired associates are presented in an order which is the same at each presentation. It is next greatest when both lists are presented in a changing order, and is absent when one list is presented in a changing order and the other in an unchanging order. The appearance and degree of retroaction is, therefore, a function of the relative order of presentation of the lists.

Experiment II

Problem and Procedure.—The degrees of learning under the 2 different presentations of Experiment I are not equal. Presentation of the items in the same order at every repetition yields larger scores on the immediate test than does presentation in a changing order. There is, as we have seen, good reason to infer that this disparity in degree of learning is not primarily responsible for the results, except possibly the difference between Work 1 and Work 3, but the validity of the inference should be tested.

In Experiment II we have repeated Experiment I with the single exception that all lists learned with a changing order have been given 8 repetitions instead of 5. All lists learned with the same order of presentation throughout have been given 5 repetitions as before. The addition of 3 repetitions increases the frequency by 60 percent and should increase considerably the degree of learning as measured by the test. The same 6 experimental conditions have been gone through in a counter-balanced practice order by 18 college students.

Results.—The number of items learned is shown in Table IV. The means under the same order are closely similar to the corresponding means of Experiment I and are an assurance of the accuracy with which the conditions of Experiment I have been repeated. The means under the changing order are uniformly higher than the corresponding means of Experiment I but are still lower than those under the same order.

TABLE IV

NUMBER OF ITEMS LEARNED UNDER EACH CONDITION. $N = 18$

List	Condition					
	Rest 1 SO	Work 1 SO–SO	Work 2 SO–CO	Rest 2 CO	Work 3 CO–CO	Work 4 CO–SO
Original: M	8.28	8.50	8.11	6.94	6.78	6.61
σM	.46	.39	.45	.47	.34	.31
Interpolated: M		8.50	7.94		7.28	8.55
σM		.43	.47		.55	.48

The fact that they are higher than those of Experiment I makes possible, nevertheless, a partial test of the influence of degree of learning upon the conclusions.

The means within each of the 2 types of presentation are not reliably different from each other. The unreliably small differences may be disregarded in interpreting the data upon retention.

The mean recall scores and the percentages of inhibition computed from them are given in Table V. The recall scores

TABLE V

MEAN RECALL SCORES AND PERCENTAGES OF INHIBITION COMPUTED FROM THEM

Condition	Mean Recall Scores		Percentages of Inhibition	
	Recall 1	Recall 2	Recall 1	Recall 2
Rest 1 (SO)	7.39±.68	8.61±.48		
Work 1 (SO–SO)	3.50±.74	8.11±.51	52.6	05.8
Work 2 (SO–CO)	6.39±.66	8.83±.43	13.5	+02.6
Rest 2 (CO)	6.06±.56	7.67±.49		
Work 3 (CO–CO)	3.83±.38	5.77±.46	36.8	24.8
Work 4 (CO–SO)	5.72±.42	7.39±.42	05.6	03.7

under Rest 1 and Work 1, which duplicate the corresponding conditions of Experiment I, are not importantly different from the corresponding means of that experiment. The percentage of inhibition in terms of Recall 1 is slightly larger than in Experiment I and in terms of Recall 2 is slightly smaller. Under Work 2, in which the interpolated list with a changing order receives 8 repetitions, the recall scores are very nearly

the same as in Experiment I. The percentage of inhibition is somewhat larger in terms of Recall 1 and the facilitation effect in terms of Recall 2 is practically the same. It may be concluded that when the original list is presented in a constant order the addition of 3 repetitions to the 5 repetitions of an interpolated list presented in a changing order does not produce any important alteration in amount of interference.

The recall scores under Rest 2 and Work 4 are, as a result of the increased frequency in learning, somewhat larger than the corresponding scores in Experiment I. Under Work 3, in which both original and interpolated lists receive the increased frequency, the recall scores are nearly the same as those of the first experiment. The percentages of inhibition are larger than those of Experiment I when both lists are presented in a changing order (Work 3) and are nearly the same when 5 repetitions in unchanging order are interpolated after 8 repetitions of an original list in a changing order.

The results of Experiment II provide an independent corroboration of two of the conclusions of Experiment I, namely, (*A*) that inhibition appears when the mode of presentation (either SO or CO) is identical at original and interpolated learning and (*B*) that inhibition is almost or entirely absent when one list, either original or interpolated, is presented in one way and the other in the second way. The third conclusion, that inhibition is greater when presentation is in the SO–SO than in the CO–CO condition, is left in question. The percentages of inhibition under Work 3 increase from Experiment I to Experiment II so that the difference in inhibition between the SO–SO and CO–CO conditions of Experiment II is less at Recall 1 and is in reverse direction at Recall 2. This may be a function of the increased degree of learning of the lists learned under the CO condition, and may mean that as degree of learning under the 2 conditions (SO–SO and CO–CO) becomes equal differences in inhibition vanish. Experiment III has been conducted to test this possibility.

We have, thus far, said nothing about relearning scores. It will be remembered that the differences between the means

of the trials required to relearn in Experiment I were, within each type of presentation of the original lists, very small. With presentation in unchanging order the means of the 2 experiments agree closely. With presentation in a changing order, the means are, as a function of the higher learning frequency in Experiment II, consistently smaller. The differences between them are of the same order of size and the relations between them have a similar implication except that inhibition is greater, though unreliably so, under Work 3 than under Work 1. They support, otherwise, the conclusions reached from the recall scores and in order to conserve space the detailed data are not reproduced.

Experiment III

Problem and Procedure.—We have used in this experiment a still higher frequency with the lists learned under the changing order of presentation in an attempt to bring the amounts learned under the 2 orders to equivalent levels. Preliminary experimentation showed that 12 repetitions under the changing order were necessary in order to obtain means approximately equal to those yielded by 5 presentations with unchanging order. We have, therefore, repeated the preceding experiments with 12 subjects in all essential respects except that each list under a changing order of presentation was given 12 repetitions. The 12 repetitions were given in 2 groups of 6 each with a two-minute rest interval, filled with conversation, between the 2 groups, *i.e.* between repetitions 6 and 7. This required 20 seconds more than a ten-minute interpolated interval and all intervals were lengthened by this inconsequential amount of time.

The use of 12 repetitions in all CO conditions in comparison with 5 repetitions in all SO conditions introduces the variable of total time spent in learning. The time spent in learning under the CO conditions is now 2.4 times as long as under the SO conditions and, when interpolated learning is with a changing order, fills the interpolated interval. We know nothing about the influence of total time spent in learning, apart from other variables, upon degree of inhibition. A comparison of the data of the 3 experiments, which

have employed 5, 8 and 12 repetitions of the lists learned under a changing order, shows no progressive change in inhibition under Work 2 or Work 4, and although the inference that none is to be expected in Work 3, therefore, is not exact, it is probable. There is, in any case, no reason to suppose that the increased time spent in learning has been the major effective variable in Work 3.

The mean number of items learned under each condition appears in Table VI. The means under SO and CO are of

TABLE VI

MEAN NUMBER OF ITEMS LEARNED UNDER EACH CONDITION. $N = 12$

List	Condition					
	Rest 1 SO	Work 1 SO–SO	Work 2 SO–CO	Rest 1 CO	Work 3 CO–CO	Work 4 CO–SO
Original	7.58	7.58	8.50	7.08	7.50	7.66
Interpolated		8.83	8.50		8.33	8.66

the same order of magnitude and the differences between them are of the same magnitude as the differences within a single order. No difference is reliable and the sigmas are not given. A frequency of 12 applied to lists learned under a changing order has, thus, yielded a degree of learning approximately equal to that from a frequency of 5 under the same order.

The mean recall scores and the percentages of inhibition computed from them are given in Table VII. The per-

TABLE VII

MEAN RECALL SCORES AND PERCENTAGES OF INHIBITION COMPUTED FROM THEM

Condition	Mean Recall Scores		Percentages of Inhibition	
	Recall 1	Recall 2	Recall 1	Recall 2
Rest 1 (SO)	7.50±.97	8.25±.80		
Work 1 (SO–SO)	3.33±.98	7.50±.73	55.6	09.1
Work 2 (SO–CO)	7.91±.69	8.75±.64	+05.4	+06.0
Rest 2 (CO)	7.16±.65	8.25±.51		
Work 3 (CO–CO)	5.08±.81	7.41±.73	29.1	10.2
Work 4 (CO–SO)	7.33±.64	7.83±.55	+02.4	05.1

centages of inhibition under Work 1 are of the same order of size as those found in Experiment II. Under Work 2 very slight facilitation effects appear, while under Work 4 Recall 1 shows slight facilitation and Recall 2 slight inhibition. When the unreliability of the differences between the means involved is considered, it can only be concluded that no effect from interpolation appears under Work 2 and Work 4. The results of Experiments I and II for Work 2 and Work 4 are, thus, corroborated.

The present experiment was performed, however, to discover whether an increased degree of learning with the changed orders of Work 3 would yield increased inhibition and particularly, an amount of inhibition equal to that produced by the constant orders of Work 1. Reference to Table VI shows that the numbers of items learned under Work 1 and Work 3 are of similar magnitudes. Percentage of inhibition, as measured in terms of Recall 1, is, nevertheless, greater under Work 1 than under Work 3. In terms of Recall 2 it is practically equal under the 2 conditions, as it was in Experiment I.

The relearning scores which, in order to conserve space, are not given, show similar tendencies. Under Work 1 the 2 relearning scores give 15.1 percent and 22.9 percent of inhibition, respectively, while under Work 3 they give facilitation effects of 09.1 percent and 16.4 percent. This cannot be taken to mean a reliable freedom of relearning from inhibition with increasing degree of learning under changed order, because in Experiment II relearning scores, which are not given here, showed a slightly higher percentage of inhibition than they did in Experiment I. The small and unreliable differences between mean trials to relearn under rest and work conditions however, removes relearning scores from primary consideration.

It can only be concluded that Work 1 yields more inhibition in terms of Recall 1 than does Work 3. The 3 experiments show contradictory relationships in terms of Recall 2 and the most reasonable conclusion is that there are no apparent differences between Work 1 and Work 3 at Recall 2.

Differences in terms of relearning scores are small and unreliable, but they agree in showing, in Experiments I and III, more inhibition under Work 1 than under Work 3.

Experiment IV

We shall report briefly the results of an experiment performed with 12 college students during the course of our preliminary work with the method of paired associates and the problem of order of presentation. The conditions of Experiment I were duplicated with the single exception that one presentation of the pairs was given at the beginning of relearning and before the test. In Experiment I, as in Experiments II and III, a recall test preceded the first relearning repetition; in Experiment IV the first relearning repetition preceded the test.

The mean learning scores closely approximate those of Experiment I. The recall scores are, as would be expected from the fact that a relearning presentation had preceded the recall test, considerably larger than those of the first experiment, and the means of the relearning trials are smaller. The differences between the percentages of inhibition have the same direction as those of Experiment I and are, in terms of relearning, more pronounced. The chief value of this experiment lies in the fact that it provides an independent confirmation of the results of Experiment I under conditions which differ only slightly from those of Experiment I.

Experiment V

Presentation in a changing order has meant in Experiments I to III that the pairs in the list occupied a different serial position at each presentation. The intra-serial order of the pairs remained unchanged except for the necessary terminal breaks at different points each time, caused by beginning each presentation with a different pair and by the reversed order. It is possible that this relative constancy of intra-serial order during the last 4 trials, with the attendant possibility of backward associations, may have affected the learning. A test of this possibility is not of crucial im-

portance but will be helpful in interpreting the difference between the amounts learned under constant and changing orders.

The 6 conditions of Experiment I were repeated by 6 college students with the single exception that in all presentations with a changing order the pairs were arranged in a new random intra-serial sequence at each succeeding presentation. Where in Experiment I the first order would be 1 to 10 and the second 5–4–3–2–1–10–9–8–7–6, for example, in Experiment V the first order would be as before, the second would be 4–8–1–5–7–3–10–2–6–9 and the 3 remaining would be in other random sequences.

In spite of the small number of subjects, the means for learning approximate closely those obtained in Experiment I. The means under the changing order are the important ones for the present purpose. They are, for Rest 2, Work 3 and Work 4, 5.66, 6.00 and 5.00, respectively. The interpolated lists in Work 2 and Work 3 have means of 5.33 and 5.16. The particular intra-serial arrangement used in Experiments I to III has not, apparently, yielded a different degree of learning than would be expected from an entirely random order. The percentages of inhibition differ in the same directions as do those of Experiment I.

Discussion

The experiments present the following results for interpretation. (1) Presentation of the pairs in the same serial position at every repetition leads to a greater degree of learning, as measured by an immediate test, than does presentation in a different serial position each time. Twelve repetitions are required under the changing orders to bring the number of items learned to an approximate equivalence to the number learned in 5 repetitions under a constant order. (2) Inhibition is greatest when both lists are presented throughout with the pairs in the same serial position (SO–SO), next greatest when the positions of the pairs of both lists change from trial to trial (CO–CO), and virtually absent when either list is presented in a constant order and the other in a

changing one (SO–CO and CO–SO). (3) Recall 2 and the relearning scores show much smaller and less reliable differences between rest and work than does Recall 1.

We shall consider first the greater rapidity of learning under a constant than under a changing order of presentation. Three possible interpretations of this fact at once present themselves. (1) The difference may be a function of the facilitating effects of associations between successive pairs. Such associations under a constant order of presentation might provide additional cues to the recall of the second members and serve as organizing factors. The subjects were instructed to connect the members of each pair and to disregard the order of the pairs. They were told, however, whether the pairs would always be given in the same or in a different order [5] and they might have disregarded the instructions and have partially learned the order. The lists under the SO conditions might, then, have been learned partially by the anticipation method as well as by the method of paired associates. This would permit, too, the formation of some remote forward and backward associations which might operate to facilitate performance at the test.

This interpretation is very plausible, but there is evidence to show that it is not the only one. Under the CO conditions the intra-serial order of the pairs was, except in Experiment V, constant after the first trial save for a break at a different point at the end of the list at each trial. The subjects had, therefore, some opportunity to form associations between successive pairs, and there was the possibility of the formation of remote forward and backward associations. When, however, the pairs followed each other in a random order at each trial (Experiment V), the amount learned was not noticeably decreased. It does not seem likely, therefore, that the difference between the learning scores under constant and

[5] It was necessary to inform the subject concerning the order because, without such information at the outset, he would observe for himself the regularity or irregularity of order after the second or third trial. Some of the subjects would observe this sooner than others and in order to make conditions as constant as possible from subject to subject, the instructions contained this information.

changing orders is wholly ascribable to associations between successive pairs. This may have contributed to the result, but it is probably not the only factor.

(2) The difference may be a function of associations formed between either or both members of each pair and absolute or relative serial position. Such associations could aid learning independently of any effects from intra-serial association between successive pairs. The constant appearance of each pair at a given serial position would permit the subject to use the serial numbers from 1 to 10 as a stable mnemonic system. There is at hand no direct evidence that this has been a major factor, but the hypothesis that it has been is plausible. With a changing order of presentation, on the other hand, the mnemonic aids are absent and there may be interference effects between associations with serial position on one trial and those on another. (3) The difference may result partly from a difference in set under the 2 conditions of presentation. With a constant order the subject knows what to expect and his set may be more stable and more effective, while with a changing order he does not know what order to expect and his set may be unstable and may easily lead to interference.

These 3 factors may reasonably be supposed to be major ones in accounting for the fact that learning is more rapid under a constant than under a changing order of presentation, although no exact relative weights can at present be assigned to them.

We shall turn to the interpretation of the fact that inhibition is highest when the orders of presentation of original and interpolated lists are as nearly identical as they can be made and that inhibition fails to appear when the orders of presentation of the 2 lists are dissimilar. It is possible to classify these data under the rubric of inhibition as a function of similarity and to treat relative order of presentation as one kind or dimension of similarity. Such a classification relates the data to an already established group of results, but it does not carry us far toward an understanding of the manner

in which relative order of presentation operates. We shall proceed to this question.[6]

Search for an hypothesis concerning the manner in which identity of order of the presented pairs operates must inquire first of all what factors are common to the 2 orders. Community of meaning or of other factors localized in the materials may be discarded since the lists were rotated in a manner to cancel out differences due to inter-list relations as such. (1) The first factor which is common to both lists, and the one which this experiment was first of all designed to study, is *the serial position of the items*. When the pairs of the original list are presented in the same order throughout, the position of each pair is a constant and there is an opportunity for each pair to become connected with either absolute or relative position or with both. When the pairs of the interpolated list are, likewise, presented in a constant order, there is an opportunity for the same kinds of connections between pair and position. On the assumption that such connections are made, Position 1 is connected first with Pair A and afterwards with Pair K, Position 2 first with Pair B and then with Pair L, and so on. This association of a single position with 2 pairs might produce some associative inhibition and, at recall of the original list, some reproductive inhibition. A comparison of the means of the original and the interpolated lists (Table I), when the two have been learned under comparable conditions (Work 1), shows that the mean of the interpolated list is lower. A similar difference does not appear in Experiments II and III, however, nor does it appear consistently in Experiments IV and V. We have, therefore, no trustworthy evidence that associative inhibition has occurred. It is possible that associative inhibition should not occur to a degree which is measurable under these conditions but that, nevertheless, reproductive inhibition should appear. It is possible, also, that associative inhibition is transitory, in that

[6] We shall omit discussion of the fact that the appearance of greatest inhibition when order is identical deviates from the results obtained with identical items. *Cf.*, for example, E. S. Robinson, The 'similarity' factor in retroaction. *Amer. J. Psychol.*, 1927, **39**, 297–312. Identity of order and identity of items belong, of course, in 2 different categories and there is no reason to expect similar results from the 2 identities.

it appears only in the very early trials, and that it had disappeared by the end of the fifth trial.

More important from the standpoint of the results upon retention is the probability that an association between a given position and a second pair, viz., the pair occupying that position in the interpolated list, will yield reproductive inhibition as shown by a weakening of the connection between the position and the first pair. To the extent that this connection has been an aid in the acquisition of the pairs, such reproductive inhibition should be at least one factor in the obtained decrement in retention. That the connection has been an aid in the learning of the lists is suggested by the more rapid acquisition under presentation in a constant than in a changing order, although other factors probably operated also. There seems to be no important degree of probability, in the light of the results of Experiment V, that specific sequence *per se* should be an inhibiting factor when there is no community of meaning between the original and the interpolated lists. We may, therefore, consider position to be a significant factor.

The hypothesis to which the consideration just stated leads is that the retroactive inhibition found in this experiment is at least in part a form of reproductive inhibition in which any given position and the pairs at that position in the original and interpolated lists are the terms involved. The connection of position with the second or interpolated term weakens the previously formed connection between that position and the first or original term.

This hypothesis is consistent with the results of Work 1, Work 2 and Work 4, since inhibition appears when there is an opportunity for a constant association between position and pair in both original and interpolated lists and does not appear when the changing order of presentation prevents the formation of a constant association. The hypothesis does not fit the results from Work 3. In Work 3 there is no opportunity in either list for the association of position and pair, and still retroaction tends to appear, although in smaller amounts than those found in Work 1.

In so far as the results of this experiment are concerned this form of the hypothesis is indeterminate. It may account for the greater inhibition under condition SO–SO than under condition SO–CO, but it does not account for the fact that inhibition is found under condition CO–CO.

(2) There is, however, a second form to be considered in which the common terms are *the set of the subject, and the specific methodological context.* According to this, the method of presentation under a given order, whether the same or changing, 'sets' the subject in a certain way and provides a methodological context in which the list is to be learned.[7] Each pair in the list appears against the background of this set of the subject and of the context and may become connected with each aspect of it. When this background is the same under both original and interpolated learning, there is opportunity for reproductive inhibition in the same way as described above, except that here the set and methodological context are the common factors. This is a reasonable interpretation but that it is the only one is called in question by the fact that Work 3, in which the changed order of presentation is used with both lists, yields less inhibition than does Work 1 in which the unchanged order is used with both. One might argue for its generality by making certain special assumptions, such as that the set and methodology are more stable and connectable terms when presentation is in a constant order than when it is changing. This is a reasonable assumption, but in the absence of conclusive evidence the most acceptable hypothesis includes both possible factors and assumes that retroactive inhibition reduces at least in part to reproductive inhibition with serial position, with set and methodological context each playing the part of common terms in the reproductive inhibition paradigm.

This hypothesis about the operation of serial position, set and methodological context as common terms in the bringing about of reproductive inhibition is, of course, only an attempt

[7] This factor may have much in common with the factor of similarity of operation studied by Gibson and Gibson and shown by them to be a condition of retroactive inhibition. See E. J. Gibson and J. J. Gibson, Retention and the interpolated task. *Amer. J. Psychol.*, 1934, **46**, 603–610.

to imagine what is involved in identity of order of presentation. Whatever the hypothesis, the facts that identity of serial order of presentation produces more inhibition than does dissimilarity of order and that identity of a constant order produces more than does identity of changing order remain. For a general theory of the place of retroactive inhibition in mental organization, the fact that relative serial order is a determining condition of inhibition is important.

It may be noted, finally, that these results offer another bit of evidence against perseveration as a major condition of retroactive inhibition. We have found that 5 interpolated presentations occupying identical times yield inhibition in amounts which vary with relative order of presentation, whereas the perseveration theory would lead us to anticipate either equal amounts of inhibition or slightly more from interpolated learning with a changing order on account of the somewhat greater effort which it requires. We have seen that in Work 2 this interpolation yields no clear inhibition. There may still be perseveration effects under all conditions, although the failure of inhibition to occur in Work 2 and Work 4 makes this very doubtful. In any case we must look elsewhere than to perseveration for the factors which differentiate between the 4 conditions. The transfer theory is the more plausible but is in its usual form too indefinite to do more than point to the general category in which we must look for an understanding of the phenomena. The hypothesis which we have sketched above states in terms of reproductive inhibition one way in which the 'transfer' required by the theory may possibly occur.

The final question to be considered is the fact that inhibition, where it occurs, is greater in terms of Recall 1 than in terms of Recall 2 or of either relearning score. This is a common result in experiments on retroactive inhibition. The facts imply that the inhibitory influence of the interpolated list decreases sharply from the first to the second relearning trial and probably very slowly thereafter. This is, in itself, of considerable importance and deserves more de-

tailed analysis than it has ever received. An adequate hypothesis about it requires more knowledge than is now available concerning the conditions of inhibition. It is readily conceivable, however, that it can be accounted for in terms of the reproductive inhibition hypothesis which has been described above.

Correlations Between the Four Measures of Retention

We have several times referred to the consistency with which the 4 measures of retention exhibit similar tendencies. The question may be raised, however, whether the four are sufficiently independent to make the fact of their agreement more convincing than any one measure alone would make it. To answer this question we have intercorrelated the four, each with every other, in each of the 6 conditions of Experiment I. Recalls 1 and 2 yield coefficients of 0.334, 0.601, 0.849, 0.802, 0.739 and 0.793 for the 6 conditions in the order in which they are listed in the tables. Relearnings 1 and 2 give coefficients which vary from 0.70 to 0.93, while the 2 recall scores show considerably negative correlations with the 2 relearning scores, varying in size from -0.47 to -0.91.

While these relationships are relatively high, they are by no means high enough to mean that the 4 scores are measuring completely identical functions. We may, therefore, regard agreement among the four, with respect to degree of inhibition shown, as significant, although less so, of course, than would be the case if the correlations were much lower.

Conclusions

1. The amount learned in 5 auditory presentations of a list of 10 paired associates is a function of the constancy of order of the pairs at successive presentations. When the order, that is, the serial positions, of the pairs is the same throughout, more is learned than when the order (serial positions of the pairs) is different at each presentation. This is true whether the order in which the pairs follow each other, regardless of serial position, is kept the same or is

varied randomly. Three factors which may possibly account for the major fact have been discussed.

2. Percentage of inhibition is a function of the relative order of presentation of the pairs in the original and interpolated lists. Inhibition is greatest when both lists are presented in a constant order at every repetition (SO–SO). It is next highest when both lists are presented in a different order at each repetition (CO–CO). The fact that it appears with some consistency points to its validity in spite of the statistical unreliability of most of the differences between Rest 2 and Work 3. Inhibition is very slight and is probably absent when one list is presented in a constant order and the other in a changing one (SO–CO and CO–SO).

3. These results are not primarily a function of differences in degrees of learning under the 2 types of presentation, nor are they a function of the intra-serial order of the items, as separate from their serial positions.

4. The data provide additional evidence against perseveration as a major condition of the inhibition. The transfer theory can more readily account for the results and an hypothesis is stated according to which the negative transfer occurs after the reproductive inhibition paradigm, with serial position, set and methodological context playing the rôle of common terms.

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THE INFLUENCE OF SUBLIMINAL STIMULI UPON VERBAL BEHAVIOR [1]

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SECTION I. INTRODUCTION

Preliminary Statement.—In a previous study which the present author prosecuted in collaboration with Arthur Silverman (**8**), an attempt was made to establish a conditioned response to a subliminal stimulus. The results of this study, while not definitive, indicated that it may be possible to form such a conditioned response. Such a conclusion, however, must be premised upon the assumption that it is possible to influence behavior by means of a stimulus which is of so small an intensity as to be below the limen of conscious activity. If this is true, then one might be justified in arguing further that conscious processes themselves, *qua* verbal behavior, may also be influenced by such stimuli.[2]

Historical.—The fact that the influence of subliminal stimuli upon verbal responses presents mechanical obstacles to investigation has discouraged its being attacked in the laboratory by more than a few students. Some few experiments, however, do present evidence pertinent to the present discussion. As early as 1863 Suslowa (**9**) showed that æsthesiometer sensibility is lowered under electrical stimulation, even when the current is subliminal. Twenty-one years

[1] The writer is indebted to Professor Hulsey Cason for assistance and counsel in carrying out the present study.

[2] The question of the existence of subliminal percepts achieved its first importance with Leibnitz's description of what he called *petits perceptions*. However, as he describes them in the *Monadology* and in the *New Essays*, his petits perceptions seem to be consciously discriminable items of experience which are not at the moment in attention. It is true that in exposing and applying his famous principle of continuity he infers the necessity for such subliminal phenomena as we are here investigating, but he appears not to differentiate between stimuli that are subliminal because of their small intensity and those that are subliminal because merely unattended to. It would seem that whether or not these phenomena are similar in kind remains to be shown.

later Peirce and Jastrow (**6**) showed that judgments of the heavier of two imperceptibly different weights were more often right than wrong although the judgments were made with zero confidence.

Sidis (**7**), in 1898, published the results of a series of investigations of this question. His experiments were performed with himself as S in one procedure and as E in others. Cards, upon which were printed numbers or letters, were held in view of the S at such a distance from him that he was just unable to read them. Upon requiring the S's to guess, Sidis found that a smaller percentage of error obtained than chance would lead one to expect. He concluded therefore that, "There is within us a secondary, subwaking, reflex consciousness," which influences these guesses (**7**, p. 179).

Whether these results are to be explained on the basis of unconscious cerebration or by resort to a subconscious is not of importance to our purpose. We are interested here only in the fact that Sidis's data indicate an effect upon behavior of a stimulus which is below the conscious-judgment limen. His experiment was not adequately controlled, however, and his results are not conclusive.

Dunlap (**4**), using a modification of the Müller-Lyer illusion, cast the angular lines as subliminal shadows and asked the S's to guess the lengths of the various lines. In this investigation Dunlap found that the imperceptible shadows did produce the effect of the illusion. However, the margin of difference in his study, though positive, is but slight and inconclusive.

Dunlap's experiment was later repeated by Titchener and Pyle (**11**) with results that gave no evidence that the imperceptible shadows exerted any influence. These authors further concluded that the degree of imperceptibility is not a factor in the problem.

Mangro and Washburn (**5**) repeated Dunlap's experiment, using a slightly different technique from that of Dunlap and of Titchener and Pyle, and interpreted their results as supporting Titchener and Pyle and failing to confirm Dunlap's findings.

In the same year (1908) Stroh, Shaw, and Washburn (**10**)

made another study of the effect on behavior of stimuli below the conscious limen. This study consisted of a series of experiments patterned after the work of Sidis. The results of this investigation agreed with Sidis's findings, and showed that, at least in some cases, the S's judgments may have been influenced in the direction of correctness even though he were unaware of the influencing stimuli.

A study reported by Coover (**2**) was also based on Sidis's technique. In this experiment there was greater control of the variables of the situation and a more detailed computation of the data than was true of Sidis's work, and these results also showed a positive effect on verbal behavior of subliminal stimuli.

Bressler (**1**) reported another experiment similar to Dunlap's in which the Müller-Lyer figure was used. In this study the arrow marks at the ends of the figure were made by means of Hering grey paper which differed to an indiscriminable degree from the grey of the background. Two different shades of grey were used, thus giving two degrees of intensity of subliminal stimuli. The S's were asked to guess the lengths of the various lines both with and without the presence of subliminal arrow marks. The results indicated that the illusion was effected by means of the subliminal stimuli, and that the magnitude of the response was a direct function of the intensity of the stimulus.

Dunlap (**3**) reported a further experiment which illustrates still another method of investigation of such phenomena. In this study it was found that physical interruption of an inaudible sound was perceived by the S's.

It can readily be seen from the experimental evidence that no definitive statement can as yet be made. There seems to be as much evidence on one side of the question as on the other. It is for the purpose of clarifying this question that we have prosecuted the present study. To this end we have investigated both the visual and the auditory modalities with stimuli of intensities both immediately above and immediately below the conscious-judgment limen.

Section 2. Apparatus

Visual Modality.—The apparatus used with the visual modality was set up in a room in which the intensity of the light could be varied directly by means of a rheostat. At one end of the room was placed E's table, which had erected upright upon it a flat piece of ordinary wall-board, 32 by 36 in. E sat behind this board which served as a screen, while S sat facing its front surface. The latter aspect of the screen was uniformly covered with a medium grey paper, and presented a circular hole, two cm in diameter, located slightly below the center of the screen and 3 ft 5 in from the floor.

Immediately behind this hole was placed a revolving disc whose axis of rotation was perpendicular to the plane of the opening at its center. Smoothly affixed to the front surface of this disc was a plain white sheet of paper upon which had been printed two red lines which intersected at right angles at the center of the aperture. These lines were printed rather than drawn, and presented a uniform appearance, having no irregularities that could be observed when closely inspected. The aspect which the screen presented to S was that of a uniform grey field in the center of which was a white circle traversed by two perpendicular red lines which were diameters of the circle. E could rotate the red lines at will and place them in any desired position.

S sat in a straight-backed chair with his head against a vertical board which was connected on its rear side with a signal key. When in its proper place, the back of S's head was nine feet from the stimulus. The source of illumination of the stimulus was placed directly above S's head and was shielded from him.

The intensity of the illumination, the vertical or diagonal direction of the stimulus lines, and a sliding screen which could be used to close the aperture, could all be operated and regulated by E behind the screen, while a telegraph-sounder, in circuit with the key behind S's head, warned E if S changed the position of his head relative to the stimulus.

Auditory Modality.—The apparatus by means of which the auditory modality was tested consisted essentially of a Seashore Audiometer, a telephone for communication from S to E, and a signal light operated by E to give instructions to S. In order to control the possibility of unintentional cues, S and E occupied separate rooms. Communication between the rooms was by means of a telephone and a signal light.

When the experiment was in progress, S sat in an easy-chair, the headphones of the audiometer adjusted to his ears, the telephone transmitter in position before his face, and the signal light before him within his field of vision.

In order to mitigate as much as possible the effects of fatigue from the weight and pressure of the audiometer head-phones, these were swung by an adjustable elastic band from the ceiling above S's head, and padded with soft sponge rubber. The pads were so constructed as to distribute the inward pressure of the phones upon the mastoid, temporal, and parietal areas of the skull and upon the lower mandible, rather than upon the soft portions of the external ear. These pads also performed the subsidiary service of damping out sounds extraneous to the situation, and of keeping the phone-diaphragms in a constant position relative to S's hearing apparatus.

The telephone transmitter into which S spoke his judgments was fixed to a crane by means of which it could be adjusted before S. The mouth-piece of the transmitter was large and megaphone-shaped, so that S needed speak only in an ordinary conversational tone to be heard by E in his room.

In E's room were situated the audiometer proper, the receiver connected with S's transmitter, and the various switches, etc. necessary to actuate the apparatus.

The audiometer consisted essentially of an inductorium shunted across an ordinary

110-volt alternating current circuit, with a motor-driven brush interrupter in its primary circuit. This interrupter, by causing a rapid series of makes and breaks in the circuit, created a sustained tone in the head-phones worn by S. The intensity of this tone could be varied in a controlled manner by varying the position of the secondary coil of the inductorium. The units of sound intensity variation for this audiometer were just less than a j. n. d. for all of our S's. E controlled the incidence of the audiometer tone by means of a switch in the head-phone circuit, which switch, being in the shunt side of the circuit, caused no observable make or break click in the phones.

The telephone transmitter of S was connected to a pair of ordinary head-phones in E's room, so that E's hands were left free at all times to operate the apparatus and record the data. The signal light in S's room had its counterpart in E's chamber and was connected in series in the same circuit, so that E could always know that the circuit was intact and S's light functioning properly. These lights were operated from a switch in E's room to establish communication from E to S by a prearranged code.

Section 3. Procedure

Visual Modality.—The following procedure was used with the visual modality.

1. S was taken into the experimental room and all external light was excluded. He was shown the apparatus and the way in which it worked.

2. S was seated in his chair, the rheostat was set at 110 on the scale used, and, while the light was left at this dim intensity for 20 min, E explained the general purpose of the experiment and the procedure that would be used, and S was given further instructions in approximately the following words.

"The purpose of this experiment is to determine the extent, if any, to which your verbal behavior may be influenced by stimuli just above, just at, and just below the conscious threshold. Since we can have no criterion of whether or not you are being stimulated except your report, we must ask that you use extreme care in the expression of each judgment.

"Our initial procedure will be a slight variation of the classical psychophysical method of minimal change. You will be seated in the chair with your head against the rest at the back and with your attention directed toward the aperture in the screen. With the shutter up so that you may see the crossed lines, we will start at a light intensity which will enable you to distinguish the lines from their white background with comparative ease, then gradually decrease this intensity until you report that the lines are no longer visible.

"We will then reverse the procedure and, starting with an intensity somewhat below this, increase the illumination up to the point where the lines just come into view. This will be repeated several times.

"The purposes of this initial procedure are: to determine roughly the position of your limen; and to allow you to become acquainted with the situation and with the way the lines appear when the light intensity is very dim.

"When several trials have been run in this manner, the real work of the experiment will start.

"The lines on the white spot can appear in either one of two positions to you—either in a vertical-horizontal position (as an upright cross), which you will designate as 'vertical'; or in a diagonally crossed position, which you will designate as 'diagonal.' It is the discrimination of these two positions at the various light intensities, *plus an estimate of the amount of confidence with which you feel that you can render this judgment*, that is your especial problem throughout the experiment. If you cannot see the lines

at all or get any cue which might aid you in making a judgment, you will then please make a guess as to their position. If you have any basis at all other than pure 'guess-work' for making these judgments, please indicate it. We ask you to be very careful in the matter of giving immediate expression to even the slightest confidence in each and every judgment.

"The procedure will be to present the stimulus by raising the screen and allowing you to look at the lines until you have uttered your judgment (either 'vertical' or 'horizontal'). After having rendered your judgment of the position of the lines, will you please, *in every case*, indicate the amount of confidence which you may have in that judgment, allowing '2' to equal great confidence, '1' slight confidence, and '0' no confidence whatever. *Only a pure guess* may be described as a judgment in which you have no confidence. All others must be described as being given with some degree of confidence—either slight or great.

"These presentations will follow each other as rapidly as the experimenter is able to record them. Please take your time and work as slowly as seems necessary to you.

"The positions of the lines at the various trials will be dictated by the order of the cards in a well-shuffled deck. Consequently, the positions will follow each other in a purely random order. Please do not attempt to guess what the next presentation may be, as this will be entirely a chance function.

"Between trials it is recommended that you rest your eyes by directing them from place to place about the grey field. Whenever you feel fatigue, please indicate it to us and we will rest for a few moments, or else discontinue the day's work if you wish it.

"Please take your time, don't tire yourself any more than you can help, and take every care to remark even the slightest confidence you may have in your various judgments."

3. S's threshold for the stimulus was then roughly determined by means of the initial procedure described above.

a. Records were made of the points on the rheostat scale at which S just lost sight of or was just able to see the crossed line as E decreased and increased the light intensity.

b. The median of the scores on the above series was found during the experiment and used as a starting point for the following procedure.

4. A series of trials was made following the procedure described in part 2 above, the stimulus being presented in the first trial at a light intensity slightly above the experimental limen determined in part 3–*b* above. At no time was the light intensity the same for two consecutive presentations, between no two presentations was this intensity varied by more than one point on our rheostat scale (one such unit being somewhat less than a j. n. d.), and progress along the rheostat scale never continued in the same direction for more than ten presentations.

For each presentation the following data were recorded: (*a*) light intensity; (*b*) correctness or incorrectness of the response; (*c*) statement of S's confidence in his judgment, '2' being used to represent extreme confidence, '1' any slight amount of confidence, and '0' absolutely no confidence, a 'pure guess.'

During some of the presentations in this part of the procedure the experimenter also recorded the actual position of the stimulus with each presentation. This was done in order to have a check later upon whether or not errors were made more frequently at one position of the stimulus than at the other.

A total of 452 such recordings were made on unselected trials on which errors were made. Of these, 225 were made with the stimulus in the 'vertical' position, and 227 with the stimulus in the 'diagonal' position. We believe that this is as good evi-

dence as can be desired that no additional cues have crept into the situation to prejudice the evidence by making the variables to be discriminated of unequal value. If there were any such cues, they were constant for both positions of the stimulus.

Auditory Modality.—The present procedure followed as exactly as possible that outlined in the first part of this study. Exact duplication of techniques was not possible, and the present procedure differed from that described above only in the following details:

1. No adaptation period was necessary.

2. The auditory stimulus was a sound which occurred in either of two temporally interrupted patterns. These patterns were the same as those described in the Morse Code as 'dash-dot' and 'dot-dash,' being respectively a long tone followed by a short and a short tone followed by a long.

3. S was instructed to attend to the auditory stimulus whenever the signal light was turned on, and to relax his attention when the light was turned off.

In this procedure also, records were taken of the frequencies of error for each of the two patterns. A total of 4000 such recordings were taken of unselected trials on which errors occurred. Of these, exactly 50 percent, or 2000, occurred with each of the two patterns. We believe that there can be no more convincing argument that no cues were operating to give the discriminables unequal value.

Section 4. Subjects

In order to safeguard as much as possible against errors being introduced into the data because of inadequacy or unreliability of the testimony, extreme care was exercised in the selection of the S's. It was felt that it would be wiser to use S's whose integrity and capability in the experimental situation we could absolutely trust, even if such selection made only a small number of S's available. The seven S's used were all advanced students in psychology, and are designated hereinafter as follows:

Visual Modality	*Auditory Modality*
1. Eloise May (Boeker) Cason	5. Margery (Hayden) Trowbridge
2. Margery (Hayden) Trowbridge	6. Neal Drought
3. John R. Knott	7. Rod Menzies
4. Joseph Runkel	8. Paul Settlage

In order to gather enough data to make our results statistically reliable, we brought the S's back to the situation a number of times. In this manner we were able to gather data on some twenty-three thousand judgments.

Section 5. Results

In computing the results, the various series of trials for each individual S were examined, and the point at which 100 percent of the 'confidence-judgments' were 'zero' (indicating complete lack of confidence at that stimulus level) was taken as the conscious judgment limen for that particular day's series. In the event, as occurred on one occasion, that this figure again became less than 100 percent at some lower intensity, the limen was considered to be at whatever lower

point the 100 percent criterion was at last arrived at and maintained. Thus, on no single presentation is a judgment upon which confidence has peen expressed allowed to be considered below the conscious judgment threshold. The occurrence of even one such judgment in a series of eighty to one hundred was enough to cause the limen to be placed at some lower point. In this manner the threshold was taken to be at the lowest point possible, and no judgment above that point, regardless of the amount of error, was considered subliminal.

With the data so arranged and the limen marked at the appropriate place, computations were made of the percentages of error at each intensity and of the percentages of 'zero-confidence' judgments at each step. The limen so determined was then taken as the base line for comparison of the

TABLE I

SUBJECT 1—VISUAL MODALITY

[Confidence was judged '2,' '1,' or '0' (pure guess)]

Units About Limen	1st Day		2nd Day		3rd Day		4th Day		5th Day		6th Day		7th Day		No. of Judgments
	Rheostat Reading at Limen														
	108		107		107		109		107		108		107		
	% '0'	% Error	% '0'	% Error	% '0'	% Error	% '0'	% Error	% '0'	% Error	% '0'	% Error	% '0'	% Error	
6	12	6	0	0	0	0	0	0	0	0	0	4	0	0	280
5	60	18	0	4	0	0	0	2	0	0	0	0	0	0	300
4	48	24	6	0	0	0	4	0	0	0	0	0	0	0	300
3	54	12	24	20	18	6	20	0	16	12	18	0	0	12	350
2	100	28	38	18	36	16	44	18	38	16	56	10	42	14	350
1	93	12	42	16	74	10	68	8	70	12	66	6	86	16	350
	Limen														
0	100	12	100	14	100	16	100	10	100	14	100	18	100	18	350
−1	100	12	100	18	100	10	100	12	100	14	100	16	100	20	350
−2	100	18	100	20	100	14	100	18	100	20	100	18	100	24	350
−3	100	36	100	28	100	24	100	22	100	18	100	24	100	24	350
−4	100	44	100	38	100	36	100	40	100	32	100	46	100	44	350
−5	100	50	100	54	100	46	100	50	100	58	100	48	100	52	300
−6	100	50	100	48	100	52									150
														Total	4130

TABLE II

SUBJECT 2—VISUAL MODALITY

[Confidence was judged '2,' '1,' or '0' (pure guess)]

Units About Limen	1st Day		2nd Day		3rd Day		4th Day		5th Day		No. of Judgments
	Rheostat Reading at Limen										
	110		112		112		110		111		
	Percent '0'	Percent Error	Percent '0'	Percent Error	Percent '0'	Percent Error	Percent '0'	Percent Error	Percent '0'	Percent Error	
6	3	11	0	0	0	0	0	0	0	0	150
5	50	10	0	0	10	0	0	0	0	0	150
4	60	0	14	0	0	0	10	0	0	0	200
3	77	22	80	0	10	10	16	0	20	0	200
2	66	11	96	24	40	10	16	0	18	10	200
1	87	24	95	20	90	0	23	16	28	18	250
Limen											
0	100	42	100	20	100	20	100	30	100	10	250
−1	100	22	100	24	100	20	100	15	100	24	250
−2	100	33	100	30	100	30	100	40	100	38	250
−3	100	57	100	50	100	40	100	58	100	46	250
−4	100	60	100	50	100	50	100	50			200
−5	100	50	100	50	100	50					150
−6	100	50									50
										Total	2550

data of various day's runs with each other. Thus, the data as finally tabulated represent the percentages of error at points 1, 2, 3, 4, 5, and 6 units above and below the limen.

Tables I–VIII contain the summarized data for each day of each S. These data are oriented with reference to the limen, and not in terms of the objective units on our rheostat and audiometer scales. In order to forestall the possible criticism that we may be thus merely expressing the variations from day to day of the subjects' temerity, rather than their thresholds for a stimulus, we have included at the top of each day's summary the rheostat or audiometer reading for that day. Thus, by adding the appropriate units to this figure, it will be possible for the reader to express any of these scores in terms of our rheostat or audiometer units. The fact that in only

one case does one of these vary from day to day by as much as three units is, we believe, a very satisfactory measure of the stability of our measure of a function which might be expected to be much more vacillating under less adequately controlled circumstances (for a single unit on either of our scales is less than a j. n. d., according to the report of these S's).

The results of the investigation of the visual modality are expressed in Tables I to IV, while those for the auditory modality appear in Tables V to VIII.

In the first column at the left of each table are recorded the units above and below the limen at which the various data occur. Each of the major vertical divisions of these tables presents a summary of a single experimental period. At the top of each of these divisions is recorded the rheostat, or the

TABLE III

SUBJECT 3—VISUAL MODALITY

[Confidence was judged '2,' '1,' or '0' (pure guess)]

Units About Limen	1st Day		2nd Day		3rd Day		4th Day		5th Day		6th Day		No. of Judgments
	Rheostat Reading at Limen												
	108		107		107		107		108		107		
	Per-cent '0'	Per-cent Error	Per-cent '0'	Per-cent Error	Per-cent '0'	Per-cent Error	Per-cent '0'	Per-cent Error	Per-cent '0'	Per-cent Error	Per-cent '0'	Per-cent Error	
6	57	28	0	0	0	0	0	0	0	0	0	0	175
5	66	0	23	7	0	0	0	0	0	18	0	10	190
4	88	22	30	7	0	0	0	12	0	0	6	0	240
3	71	14	100	28	35	6	18	0	12	6	0	0	300
2	75	37	92	33	45	19	24	18	20	14	12	20	300
1	60	30	93	21	80	6	60	20	36	28	34	28	300
Limen													
0	100	50	100	28	100	12	100	20	100	18	100	7	300
−1	100	44	100	42	100	32	100	28	100	24	100	20	300
−2	100	23	100	50	100	40	100	42	100	38	100	36	300
−3	100	54	100	32	100	48	100	50	100	42	100	38	300
−4			100	46	100	50	100	50	100	58	100	50	230
−5			100	60			100	52	100	50	100	48	180
−6			100	58			100	46			100	50	147
												Total	3262

TABLE IV

SUBJECT 4—VISUAL MODALITY

[Confidence was judged '2,' '1,' or '0' (pure guess)]

Units About Limen	1st Day		2nd Day		No. of Judgments
	Rheostat Reading at Limen				
	116		116		
	Percent '0'	Percent Error	Percent '0'	Percent Error	
6	60	0	10	0	70
5	16	16	10	0	78
4	33	16	25	10	90
3	33	0	40	0	100
2	84	33	60	27	100
1	55	50	60	40	100
Limen					
0	100	10	100	12	120
−1	100	16	100	18	120
−2	100	50	100	24	100
−3	100	15	100	30	100
−4	100	33	100	40	100
−5	100	33	100	50	100
−6	100	50	100	50	100
				Total	1278

audiometer, reading at which the limen occurred on the day treated in that column. In the column to the left in each of the major divisions are represented the percentages of 'zero-confidence' judgments, while in the right-hand column in each case are recorded the percentages of error at each stimulus intensity. The final column at the right gives the total number of judgments made by that S at each intensity. Thus, in Table I, subject 1, being tested in the visual modality, is shown to have made 12 percent incorrect judgments on the first day at a rheostat reading of 108, which point represented her limen for that day, since at that point her confidence judgments became 100 percent 'zero.' Similarly, Table V shows that subject 5, being tested in the auditory modality, made 10 percent errors on the first day at an audiometer

reading of 19, which reading was 1 unit above her limen for that day.

In Table IX, the data for both modalities are brought together to show the effects of treating them in a single group. The two modalities tested are arranged in the two major vertical divisions of this table. In each of these divisions the column on the immediate left gives the total numbers of judgments of all S's at each unit about the limen. The column on the right in each case records the percentages of error at each step. These percentages were computed by dividing the total number of errors made by all S's at each of these steps by the total number of judgments rendered by all S's at that step.

All of these tables show surprisingly large and consistent differences from chance expectancy, and the results support

TABLE V

SUBJECT 5—AUDITORY MODALITY

[Confidence was judged '2,' '1,' or '0' (pure guess)]

Units About Limen	1st Day		2nd Day		3rd Day		4th Day		No. of Judgments
	Rheostat Reading at Limen								
	18		18		18		18		
	Percent '0'	Percent Error	Percent '0'	Percent Error	Percent '0'	Percent Error	Percent '0'	Percent Error	
6	0	0	0	0	0	0	0	0	182
5	14	0	0	0	0	0	8	0	195
4	17	8	21	6	12	2	0	0	215
3	20	7	12	10	18	3	14	5	245
2	28	25	11	11	20	11	23	0	265
1	31	10	26	13	23	10	25	10	275
Limen									
0	100	17	100	17	100	13	100	17	275
−1	100	17	100	17	100	17	100	24	275
−2	100	29	100	33	100	33	100	32	275
−3	100	41	100	40	100	40	100	39	265
−4	100	47	100	41	100	49	100	48	250
−5	100	50	100	50	100	50	100	50	235
−6	100	50	100	49	100	49	100	49	200
								Total	3152

TABLE VI

SUBJECT 6—AUDITORY MODALITY

[Confidence was judged '2,' '1,' or '0' (pure guess)]

Units About Limen	1st Day		2nd Day		3rd Day		4th Day		No. of Judgments
	Rheostat Reading at Limen								
	17		16		16		16		
	Percent '0'	Percent Error	Percent '0'	Percent Error	Percent '0'	Percent Error	Percent '0'	Percent Error	
6	0	0	0	0	0	0	0	0	184
5	0	0	8	0	0	0	0	0	195
4	12	0	0	0	6	0	0	0	215
3	11	11	8	10	16	1	10	0	245
2	24	12	12	10	26	6	19	7	265
1	38	14	32	15	40	13	28	15	275
Limen									
0	100	17	100	13	100	11	100	14	275
−1	100	15	100	16	100	13	100	14	275
−2	100	29	100	25	100	42	100	16	275
−3	100	48	100	40	100	33	100	41	265
−4	100	47	100	51	100	45	100	48	250
−5	100	51	100	51	100	48	100	49	235
−6	100	49	100	50	100	51	100	50	200
								Total	3134

the conclusion that the subliminal stimuli used had an influence on the conscious judgment process.

Furthermore, the increase of the percentages of error below the limen is in almost all cases orderly and consistent, arriving at the chance limit (almost 50 percent) by gradual increments and thereafter remaining at that limit. This characteristic of the data lends strong support to the conclusion that the demonstrated influence on verbal behavior is a function of the intensity of the subliminal stimulus.

SECTION 6. SUMMARY AND CONCLUSION

This investigation has been concerned with the determination of whether or not there may be an effect upon verbal behavior of stimuli below the conscious judgment limen. Two

sensory modalities, vision and audition, were examined with reference to this function.

The method used to test the visual modality was: to present to S a pair of crossed lines which could appear to him in either of two positions and require him to utter a judgment of their position and an estimate of his confidence in the correctness of his judgment. The intensity of the illumination of the stimulus was varied from trial to trial in such a way that on many occasions S was unable to see the stimulus at all, and so on these occasions the judgments were said by S to be pure guesses. The percentages of error were then computed and found to be grossly different, even with the stimulus of subliminal intensity, than chance would lead one to expect.

TABLE VII

SUBJECT 7—AUDITORY MODALITY

[Confidence was judged '2,' '1,' or '0' (pure guess)]

Units About Limen	1st Day		2nd Day		3rd Day		4th Day		No. of Judgments
	Rheostat Reading at Limen								
	20		17		18		17		
	Percent '0'	Percent Error	Percent '0'	Percent Error	Percent '0'	Percent Error	Percent '0'	Percent Error	
6	0	0	0	0	0	0	0	0	182
5	48	0	0	0	26	0	0	0	195
4	34	0	0	0	28	0	0	0	215
3	36	4	12	5	28	4	8	2	245
2	39	11	17	7	37	5	20	6	265
1	63	14	28	15	40	15	38	12	275
	Limen								
0	100	17	100	16	100	16	100	16	275
−1	100	20	100	18	100	19	100	19	275
−2	100	29	100	28	100	28	100	28	275
−3	100	40	100	39	100	39	100	37	265
−4	100	47	100	50	100	49	100	49	250
−5	100	50	100	52	100	51	100	50	235
−6	100	49	100	51	100	50	100	50	200
								Total	3152

TABLE VIII

SUBJECT 8—AUDITORY MODALITY

[Confidence was judged '2,' '1,' or '0' (pure guess)]

Units About Limen	1st Day		2nd Day		3rd Day		4th Day		No. of Judgments
	Rheostat Reading at Limen								
	15		15		15		15		
	Percent '0'	Percent Error	Percent '0'	Percent Error	Percent '0'	Percent Error	Percent '0'	Percent Error	
6	0	0	0	0	0	0	0	0	182
5	0	0	0	0	0	0	0	0	195
4	12	10	6	0	0	0	8	0	215
3	12	8	8	8	10	3	8	6	245
2	19	12	20	11	18	7	23	7	265
1	34	14	28	17	31	12	37	17	275
Limen									
0	100	13	100	14	100	12	100	13	275
−1	100	18	100	17	100	18	100	18	275
−2	100	29	100	28	100	42	100	29	275
−3	100	38	100	40	100	47	100	33	265
−4	100	46	100	48	100	39	100	50	250
−5	100	51	100	48	100	53	100	51	235
−6	100	49	100	52	100	50	100	49	200
								Total	3152

The method used to test the auditory modality was essentially the same as that described for the visual modality, except that here the stimuli to be discriminated were sounds of controlled intensity and temporally sequential pattern. Here again the percentages of error in judgment when the stimulus was subliminal were found to be less than chance expectation.

Since errors at and slightly below the conscious judgment limen were significantly less than fifty percent of the total number of guesses, and since these percentages of error become larger below the limen as the intensity of the stimulus is decreased, the following conclusions seem strongly indicated by our data:

1. Evidence in both the visual and auditory modalities, from the results of all subjects combined, and in the case of

TABLE IX

SUMMARY OF RESULTS ON ALL SUBJECTS, BOTH MODALITIES

Units About Limen	Visual		Auditory	
	No. of Judgments	Percent of Error	No. of Judgments	Percent of Error
6	675	2.45	730	.00
5	718	4.25	780	.00
4	830	4.50	860	2.84
3	950	7.40	980	5.60
2	950	12.80	1060	9.00
1	1000	15.00	1100	12.64
Limen				
0	1020	19.00	1100	14.00
−1	1020	21.50	1100	18.40
−2	1000	33.00	1100	30.80
−3	1000	36.90	1060	38.25
−4	880	48.00	1000	46.45
−5	730	47.00	940	50.64
−6	447	54.44	800	49.20
Total	11,220		12,610	

each individual subject, gives strong grounds for the conclusion that conscious verbal behavior is influenced by stimuli which are below the conscious judgment threshold.

2. It follows that the physiological thresholds are not coincident with, but are lower than, the thresholds of conscious judgment.

3. Evidence in both the visual and auditory modalities, and from all of the subjects used, gives strong grounds for the conclusion that the influence of the subliminal stimuli is a function of the intensity of the subliminal stimulus.

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